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NATIONAL SCIENCE FOUNDATION

THE EMPIRICAL IMPLICATIONS OF THEORETICAL MODELS

Arlington, Virginia

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1 P R O C E E D I N G S

2 MR. SCIOLI: Thanks to everyone
3 for their promptness in arriving this
4 morning. We're delighted to have you all
5 for this very important program that Jim
6 Granato has coordinated from the moment he
7 landed at NSF last January. Talk about fire
8 in the belly, it's been a tremendous
9 inspiration to me to see how excited he is
10 and how excited all of you are about this
11 opportunity to share with us your thoughts.

12 A couple of bureaucratic items.
13 If we could first have our representative
14 introduce herself, and please tell us what
15 you'll be doing and what you need from us.

16 MS. GRAY: Okay. My name is
17 Irene. I'm going to be recording the whole
18 meeting and then NSF is going to have a
19 transcript prepared with everything that's
20 said here. So if everyone can make sure
21 that they address the microphone, they speak
22 only one at a time, I know that's difficult,

1 but I can only record one voice at a time.
2 And remember, all these mikes are going all
3 the time, so if you want to have a side
4 conversation, turn away from them. But make
5 sure if you want it to be recorded that it
6 goes into the microphone. Thank you. And
7 audience members, if you want to make a
8 comment, go to the mike and tell me who you
9 are.

10 MR. SCIOLI: I guess it's clear,
11 therefore, that if you have anything that
12 you do not want recorded as part of the
13 official minutes of this program, if you
14 have any proprietary rights on a thought or
15 a method or an equation, once you say it
16 here it becomes part of the public domain.

17 (Laughter)

18 I hope you know Jim Granato. Jim,
19 do you want to --

20 MR. GRANATO: I'd like everyone to
21 introduce themselves -- start on my left --
22 for the record.

1 MR. FREEMAN: John Freeman,
2 Minnesota.

3 MR. BRADY: Henry Brady, from the
4 University of California at Berkeley.

5 MS. MORTON: Becky Morton, and I'm
6 in transit between the University of Houston
7 and Whitehue (?).

8 MR. ALT: Jim Alt, Harvard.

9 MR. KEECH: Bill Keech, Carnegie
10 Mellon.

11 MS. EAVEY: Cheryl Eavey, NSF.

12 MR. McKELVEY: Richard McKelvey,
13 Cal Tech.

14 MR. YOUNG: Peyton Young, Johns
15 Hopkins.

16 MS. ZINNES: Dina Zinnes,
17 University of Illinois.

18 MR. STRAF: I'm Miron Straf with
19 NSF, on leave from the National Academy.

20 MR. ALDRICH: John Aldrich, Duke.

21 MR. ACHEN: Chris Achen, Michigan.

22 MR. BRADBURN: Norman Bradburn,

1 NSF, on leave from the University of
2 Chicago.

3 MR. BUTZ: Bill Butz, NSF.

4 MR. SCIOLI: And I'm Frank Scioli.

5 Of course, I know so many of you from
6 panels, either the science panel --

7 John Freeman commented on coming
8 up to room 1235 that things were far
9 different from 1800 G Street. I think one
10 panel meeting, because of some problems in
11 the building, we met in a room that was
12 about one-quarter this size. So we
13 certainly didn't need microphones.

14 Let me just say a word first to --
15 for those of you who did not know, Bill Butz
16 is leaving us in about 2 weeks. He has been
17 extremely supportive of this activity in
18 which you're participating today, has been a
19 great facilitator. Those of you in academia
20 know that without facilitators, we in the
21 trenches cannot do very much. So we'd like
22 to wish Bill good luck and thank him for

1 being a strong supporter of this particular
2 activity.

3 And to say to those of you who are
4 not familiar with NSF, which is just about
5 none of you, that we're delighted to have
6 Norman take time from his very busy
7 schedule. I think he appreciates the
8 importance of the topic and in his
9 introduction of himself noted that he was on
10 leave from the University of Chicago. It's
11 delightful for us to have a professor, a
12 provost, a true scholar working with us on a
13 day-to-day basis. And he's been extremely
14 supportive of this activity as well.

15 For those of you who don't know,
16 Miron's the current president of the
17 American Statistical Association. So he
18 will be one of the few persons in the room
19 who is above us in terms of his statistical
20 sophistication, I'm sure.

21 MR. STRAF: Maybe on the side.

22 MR. SCIOLI: Cheryl, as I'm sure

1 you know, is the head of MMS?

2 MS. EAVEY: Very good.

3 MR. SCIOLI: Measurement --
4 Methods, Measurement and Statistics.

5 MS. EAVEY: Close.

6 MR. SCIOLI: We change the names
7 around here, but the intention is the same.
8 Cheryl's been a big co-supporter with
9 Political Science, as you know, of many
10 activities that have to do with measurement
11 and methods.

12 And throughout the afternoon
13 you'll see other familiar faces, I hope, of
14 NSF staff. Jim's workshop has generated
15 quite a bit of interest and activity.

16 We don't quite know where we're
17 going to wind up with this. We have some
18 ideas and, perhaps by the end of the day
19 tomorrow, we'll share them with you if we
20 get some sense as to what kind of enthusiasm
21 you have for this particular activity.

22 You should have in front of you a

1 workshop agenda for today, and as you can
2 see, we have a fairly tight schedule from
3 now until, oh, just about 6 p.m. this
4 evening. There are some breaks scheduled
5 throughout. If you need directions around
6 the building, we'll be happy to give those
7 to you. There are pay phones directly
8 outside this room. And if you need to get
9 to your e-mail, we can make arrangements for
10 that as well.

11 We have a group dinner planned, as
12 you will see on page 2 of the agenda, for
13 7 p.m. this evening at a fairly nice and
14 tasty Italian restaurant just a few blocks
15 from NSF. I don't think you'll be
16 disappointed. I certainly encourage all of
17 you to come to that and to informally build
18 on what was discussed today.

19 Tomorrow morning, we hope to
20 resume at 8:30, and certainly should be done
21 by noon. If you have particular travel
22 needs that necessitate you leaving before

1 then, just let Jim or me know and we'll be
2 happy to facilitate whatever we can.

3 Jim has some opening remarks, and
4 then we'll get right to the business.

5 Jim reminded me that perhaps you
6 two would like to make some opening
7 comments. Do you have slides?

8 MR. BUTZ: Well, as a matter of
9 fact, I do have slides.

10 Now, who here can tell me what
11 this thing is, in the NSF context? What is
12 that called -- folder?

13 SPEAKER: We call it a jacket.

14 MR. BUTZ: And even though NSF is
15 going electronic, we still have these
16 things. In my 6 years here, I have signed
17 16,000 of these things.

18 SPEAKER: Did you count them?

19 MR. BUTZ: No -- well, actually,
20 16,037. In 4 of these years, I had
21 responsibility for all of the social,
22 behavioral, and economic programs, so that's

1 -- science and archaeology and all of that;
2 and then the last two, the social and
3 economic. But 16,000 in all.

4 And when I go off and talk to
5 people, give university lectures, my most
6 popular slide is this one. And the title of
7 it is, "How to Get Zero Money from NSF."
8 And I want to show this to you because I
9 think it bears on the topic. One of the
10 things that your papers are talking about,
11 and that is how unique is the issue in
12 political science that you're talking about.

13 Now, these 16,000 proposals I
14 haven't gone through systematically and,
15 sort of, checked everything. But one thing
16 that I look at always is this so-called
17 Form 7 on the left-hand side, which is the
18 program officer's reason for turning down
19 the proposal, or funding the proposal. The
20 Form 7, and there it is for this particular
21 proposal. And frequently I'll read some
22 other part, read the reviews, maybe read a

1 little bit of the proposal, maybe read the
2 whole proposal. But I always read the
3 Form 7.

4 And of those 16,000, about 2 years
5 ago I formulated just a sort of a stylized
6 FAQ what the principal ways are to be sure
7 that you don't get money from NSF. And out
8 of all the possible reasons, there were
9 three that came to the front. And since
10 that time, I've been, sort of, paying more
11 attention to it and my strength of surety of
12 these reasons has only increased.

13 Now, it varies some across fields.
14 And I don't mean to say that this is
15 particularly true of political science, but
16 I want to show it to you because it may give
17 you an additional context for the reasons
18 why scientific proposals fail in the social
19 and behavioral sciences -- how to get zero
20 money.

21 The first and most important, the
22 modal reason for failure is something to do

1 with the theory or the conceptualization.
2 And in that, the most frequent problem is
3 that there just isn't any. The proposal
4 starts and it says, you know, I'm going to
5 run an experiment on such-and-such, and
6 here's the sample, and here's the controls.
7 Or I'm going to do a survey or something.
8 But there's just no conceptualization, no
9 sense of how what this person is doing fits
10 into what came before conceptually or how
11 the results, if they're confirmed or not
12 confirmed, will feed some kind of a general
13 conceptual sense of what's going on.

14 The second most common problem is
15 that there is one, but it's not connected.
16 One, for example, sees this frequently in
17 economics, that there will be a
18 well-developed deductive theory at the
19 beginning, and then the next section will be
20 data, the next section will be empirical
21 equations, and you'll look at the empirical
22 stuff and it's just -- it's not connected,

1 or it's only connected in the vaguest sense.

2 And a third and much less frequent
3 problem is that there's a theory there but
4 it's incorrect. There's something wrong
5 with it. It's either deductively flawed or
6 in some other way wrong.

7 The second most important reason
8 and I'm going to skip over this one because
9 this is less important for this context, has
10 something to do with the importance of the
11 proposal. The most frequent problem, as you
12 can suspect, is that it isn't important.

13 People ask, well, what difference
14 will it make if these people actually find
15 what they think they're going to find? And
16 they go around the table and say, well,
17 there's nothing in the proposal about it and
18 we don't think it's important at all.

19 The second-most is that the case
20 is made but it isn't understandable. And
21 I'll not dwell on this any further. But the
22 the third most frequent problem has

1 something to do with the experimental or the
2 data or the statistical methods. And so
3 it's sort of this and the first one that
4 relate to some of the things that you're
5 going to be talking about here today.

6 And the most frequent problem is
7 "inadequately specified." I don't know how
8 many panels I've sat in where people say,
9 well, you know, we can't really tell how
10 they're going to form this proxy from these
11 variables, or we can't really tell how
12 they're going to get over the statistical
13 problem with such-and-such. And somebody
14 around the table will say, oh, well, you
15 know there's actually a very good way to do
16 that -- you do so-and-so and so-and-so and
17 so-and-so. And somebody else at the table
18 says, well, that's great, you write the
19 proposal and we'll fund it.

20 So inadequately specified,
21 inappropriate, or out-of-date.

22 There are many other things that

1 are wrong with proposals, but these two --
2 something wrong with the theory and
3 something wrong with the data or the
4 statistical methods are two of the three
5 most common ones across -- and I really
6 don't think there are very many exceptions
7 to this -- across the 18, I think now 19,
8 programs in the social, behavioral, and
9 economic sciences here. So I thought I
10 would just point that out.

11 I want to also say that when I
12 recruit program officers at NSF, one of the
13 things I tell them is that this job as
14 program -- and I guess I'm recruiting even
15 now by saying this -- that this job of
16 program officer is arguably the most
17 powerful science job in the world, NSF
18 program officer. And the reason is not that
19 the program officer has most of what to say
20 about whether this jacket gets funded or
21 not, the reason is what's happening here
22 today.

1 The program officer can come in
2 here with an idea of something that's going
3 on, or not going on, in their science at the
4 core or the fringe somewhere and has the
5 tools to make it happen. And can start a
6 fire that their successors can't put out.

7 Sometimes that works and sometimes
8 you're just pushing on a string because the
9 field isn't ready for it yet. And what we
10 see here today is that kind of result, of
11 Granato coming in here with an idea that
12 really resonated with Frank Scioli. And
13 getting you all here and I judge from the
14 papers it resonated with you, too. And
15 we'll see in the succeeding year or 2 or 3
16 whether this is pushing on a string or
17 whether it's really lighting a fire.

18 But my hope is and my fond
19 expectation is that my successor 2 or 3
20 years from now, when he or she recruits a
21 program officer, will be able to use today
22 and tomorrow morning as an example of what a

1 program officer can do.

2 MR. SCIOLI: Thank you, Bill.
3 Norman.

4 MR. BRADBURN: Thank you. I
5 wanted to say a little bit about the context
6 within which NSF is operating at the moment,
7 or at least the directorate is operating in,
8 and how this conference fits into what I
9 hope happens in the next few years.

10 First of all, let me just, in the
11 spirit of truth in advertising, I'd like to
12 say that I am a fox but a closet hedgehog.
13 But primarily, in the fox and hedgehog
14 distinction. Most of my life has been foxy.

15 As many of you know, and some of
16 you have participated already in other
17 activities related to this, I came to NSF
18 last year to help design what was then
19 called an initiative, but with the change of
20 administration is now called a priority
21 area, for a major investment in the social
22 and behavioral sciences in fiscal 2003,

1 which at that time I thought was a long time
2 away. But it's now upon us. And the next
3 few months, in fact, will be the most
4 important time for that because we are
5 putting together the NSF budget request for
6 fiscal 2003 this moment.

7 There is a strong commitment on
8 the part of the senior management at NSF,
9 and particularly the director and the deputy
10 director, to make in 2003 social and
11 behavioral sciences a priority area, which
12 translates into a disproportional increase
13 in our budget. Now, exactly how -- it's
14 still a little bit unclear exactly how this
15 will be structured, but -- and partially --
16 and also the level, because as you know, we
17 had a little setback in expectations based
18 on the 2001 budget, where NSF got a very
19 substantial increase and commitment from the
20 Senate, at least, to being on a doubling
21 track over the next 5 years.

22 Because of the change of

1 administration and the 2002 budget, which is
2 basically a stand-still budget or a
3 half-step backward budget, I think is
4 perhaps the more realistic way of talking
5 about it, at least the way it is right now,
6 we haven't had the markup of it. We are
7 told by people who are supposed to know
8 these sort of things -- you know, the
9 unnamed sources -- that we may get a fairly
10 substantial increase over what the President
11 requested. But that won't be apparent until
12 the very end of the fiscal year, I suspect.

13 So now the thinking is still --
14 still the aspiration is to double the NSF
15 budget in 5 years. We just sort of slipped
16 the years over in taking 2002 as the base
17 year, which is actually slightly beneficial
18 because 2001 was such a big increase. So
19 we're starting talking about doubling from a
20 higher base.

21 But the big question will be how
22 big a priority area will be the social and

1 behavioral sciences and will the
2 administration and Congress buy it. It's
3 not enough just to have the director and the
4 deputy director and everybody behind it, but
5 we also have to sell it to OMB and to
6 Congress.

7 So what I've been doing with
8 Marin's help, who came over for a year from
9 the academy to help me do this, was to try
10 to put together the case for a substantial
11 investment in social and behavioral
12 sciences. And we were making the case on
13 basically two grounds -- perhaps the
14 traditional two, in some sense: It's
15 needed, and it's particularly important at
16 this time. And we're basically hooking on
17 the fact that there have been enormous
18 changes in society as a result of the whole
19 range of new technologies; information
20 technology, biotechnology, et cetera,
21 communications technologies, et cetera, et
22 cetera -- and that these are having all

1 kinds of effects which are on social
2 arrangements, the social framework.

3 In the end you can do a lot of
4 things at the individual level in terms of
5 psychology and cognitive studies,
6 linguistics. Not all, but most of the areas
7 in social and behavioral sciences have
8 things that can be done partially because of
9 these changes as a consequence, because
10 they're new methods of doing things, but
11 also, particularly on the social side,
12 economic side, there are a lot of problems
13 that need to be worked on more intensively.

14 But what I think is more relevant
15 to this particular conference is the second
16 part of the argument, which is that the
17 sciences are poised to make significant
18 discoveries in lots of ways, or at least
19 advance knowledge, partially because of new
20 methods and new data, and I hope that maybe
21 because of new theories, or at least the
22 bringing together of theory and data.

1 We've been making the case, I
2 think, more heavily on the new data and new
3 statistical tools side than we have on the
4 new theory side. In fact, on that side we
5 have more, I think, made the case on the
6 challenges to perhaps traditional economic
7 theory not so much, at least overtly, in
8 terms of political theory, but more on the
9 data side.

10 But I think this -- just to pick
11 up on Bill's summation -- the joining of
12 good theory and good data is particularly
13 important, I think, if we're going to make
14 some kind of progress in knowledge.

15 Now, let me just mention the three
16 things that from my point of view are sort
17 of background context, which don't -- you
18 don't need to focus entirely -- not
19 entirely, obviously, but I hope you'll keep
20 in the background when you're thinking about
21 your discussion today and in terms of one of
22 the things I would like to draw out of it.

1 One is there's a great deal of
2 interest throughout the foundation, in all
3 the fields, in what are the infrastructure
4 needs for sciences over the next 10 years.
5 Actually, the National Science Board has
6 asked each of the directorates to address
7 that sort of issue. And there are really
8 two parts to that issue that -- one has to
9 do with what are the, sort of,
10 infrastructure things that one would
11 propose. And those -- I would classify
12 those into four types.

13 One, are data platforms, the kind
14 of things like the National Elections Study
15 that is where their actual data is being
16 collected, which is sort of public use.

17 Shared facilities, which we have
18 been funding in our infrastructure
19 competitions a couple of things which, for
20 example, one will make survey capabilities
21 available for small studies, for
22 methodological studies, for pilot studies on

1 a quick-turnaround basis available to
2 people, we hope at a reasonable cost. But
3 it's, in any case, to make available small
4 survey capabilities -- survey capabilities
5 for small studies.

6 And the second thing is creating,
7 sort of, larger laboratories for
8 experimental economics or experimental games
9 of various sorts -- scaling up some of the
10 things that have occurred in -- that have
11 been done with, say, 10 or 12, 15
12 participants, scaling this up to 100 or 150
13 or something like that. And some other
14 things like that, but basically it's the
15 possibility of facilities which -- for data
16 collection, that can be shared by many
17 investigators who, at their own university,
18 can't afford to have that kind of facility.

19 The third one, of course, is the
20 one we've invested in a lot over the years,
21 and that's data archives, now extended to
22 what are called digital libraries, which

1 include, essentially, other kinds of data
2 than statistical data, characteristics and
3 so forth -- that is, brain imaging, samples
4 of languages, audiovisual archives of
5 structured interactions of different types
6 of things. But making use of new
7 technologies for storing archives of
8 information in digital form. And that, of
9 course, is something that we've been doing
10 in the past.

11 And finally, we've kind of -- in
12 these not elsewhere classified kind of --
13 but what I think of as centers for
14 developing certain areas of research. We
15 funded one spatially integrated social
16 science at the University of Santa Barbara,
17 which is, together with some geographic
18 information system, mapping libraries and
19 other things, an Alexandria project, if you
20 know about that.

21 But this is a center which in fact
22 develops some infrastructure, the sorts of

1 software for handling spatial data and other
2 kinds of training aspects.

3 So that just is your idea of the
4 kinds of range of sorts of things that I
5 would classify as kind of infrastructure.

6 Now, one is what are the things
7 that we are investing in. The other sort of
8 thing is how do you go about deciding what
9 to invest in. And that's the most difficult
10 problem for, I think, the social and
11 behavioral sciences because we don't have
12 any good mechanism at present for getting a
13 community together and, sort of, mapping out
14 what are its highest-priority needs.

15 I just had to write a thing for
16 the National Science Board to say how did
17 the Social Sciences go about deciding what
18 we've done? And my answer was at least so
19 far, or traditionally, it's been basically
20 that the sort of entrepreneurial scholar who
21 has had an idea for something that's needed
22 has corralled his colleagues, or her

1 colleagues, to band together to pressure us
2 or others and written a great proposal, so
3 forth, and convinced us or others to support
4 it.

5 But it's been a sort of --
6 primarily, I think, an individual or
7 small-group enterprise. And maybe it will
8 always be that way, I don't know.

9 The thing which is always around
10 here contrasted with are the astronomers,
11 who of course have very large infrastructure
12 needs. And they do, as a community, band
13 together and lay out a set of priorities,
14 you know, in the long run if they want first
15 this type of telescope, then that type of
16 telescope, et cetera.

17 And they've managed to corral the
18 people who want different types of
19 telescopes and so forth to come to an
20 agreement on what the priorities are, and
21 then they march in to NSF and to NASA and to
22 Congress, and look very well organized and

1 as if they know where they're going and so
2 forth, and make their case.

3 And that is very impressive to
4 people -- "people" being OMB-type budget
5 people, Congressional staff, and so on and
6 so forth. And they say to others, not just
7 to social and behavioral sciences, but to
8 physics and chemistry and geosciences and
9 biology and so forth, you know, why can't
10 you get your act together if you want these
11 kind of infrastructure things in the same
12 way?

13 Well, I think there are lots of
14 reasons, not the least of which is that the
15 needs for the other sciences are much more
16 heterogeneous than the range of telescopes
17 that run from optical to infrared to
18 terrestrial and space. So that's a -- it's
19 very expensive, but the range of variation
20 is pretty small.

21 Any case, I think any help on that
22 sort of score that you can get us would be

1 useful.

2 Two other things that I'm building
3 into the, kind of, plan are for training.
4 What kind -- I think -- I've been involved
5 in training programs over the years. I
6 think we need a much greater investment in,
7 particularly, quantitative training for
8 social scientists -- not just political
9 scientists; perhaps theoretical training.
10 But I think we need to invest in that. And
11 again, the question is where are the places
12 that we should be making our investments in
13 that kind of area?

14 And that's related to the final
15 thing, which is -- again, it's a theme that
16 runs across the foundation and all the
17 scientists -- is in research that's done
18 because of this increasing complexity,
19 technical in some respects, but it's
20 commented on in many of your papers about
21 the kind -- but if you're really going to
22 have high-level training in statistics and

1 math and you're going to have high-level
2 training in theory and so forth, given the
3 amount of time that people are in graduate
4 school and so forth, it's hard to get any
5 one person to have all of these traits at
6 the highest level; which suggests that what
7 you need, of course, are people to work
8 together in teams and so forth, people who
9 are well trained as theorists working with
10 people who are well trained as empiricists
11 of various sorts.

12 That's certainly a theme which you
13 see in lots of NSF program announcements if
14 you look at -- and certainly a theme that's
15 being developed in lots of different areas.
16 And that's, to my mind, in some sense tied
17 with the training, because one of the things
18 that I've observed over the years in my own
19 work -- and now since I've been here, Miron
20 and I have been going around talking to
21 people at different universities about how
22 you do collaborative work and how hard it is

1 to work with people from different
2 disciplinary orientations, even within your
3 own discipline if they're different
4 theoretical or intellectual styles, but also
5 across disciplines -- economics and
6 political science, or economics and
7 sociology, sociology and political science,
8 et cetera -- let alone working with
9 biologists or general scientists or computer
10 scientists or whatever -- that is, the
11 problem of how people from different
12 disciplines come to be able to work together
13 productively, it seems to me, and what kind
14 of training you need for that, if there is
15 any.

16 It seems to be an issue that we
17 need to confront. And since another one of
18 the things, in looking at the papers and so
19 forth, is this difference between people who
20 are oriented towards theory and those who
21 are oriented towards more empirical,
22 statistical things, that's in the first

1 instance, clearly, how do you get people
2 like that to work together productively?

3 So those are the kinds of
4 contextual things which I think are going on
5 here, and this, I think, is a very important
6 workshop to address many of these issues,
7 although it needn't be the primary focus of
8 what you're talking about. But I do hope
9 from the day and a half we can extract some
10 things relevant to these issues.

11 MR. SCIOLI: Thank you, Norman.
12 First let's say hello to Carl Simon.

13 MR. SIMON: Sorry I'm late. I
14 sort of had the air trips from hell the last
15 couple of days.

16 MR. SCIOLI: Any questions for
17 Bill or Norman before we begin? Anything
18 about NSF you'd like to know, or --

19 MR. YOUNG: Can I just ask one
20 question? Are transcripts from this going
21 to be available, or parts of it, to this
22 group afterwards? Or how much do we need to

1 scribble, I guess, is what I'm really
2 asking.

3 MR. GRANATO: It will be
4 available.

5 MR. SCIOLI: We will promise a
6 report within about 3 weeks after this.

7 MR. GRANATO: Yes, I think so,
8 about 3 weeks. And we will have a
9 transcript, so if you'd like copies of that,
10 we could run that off as well.

11 MR. SCIOLI: So the purpose of our
12 engaging with a recorder is so that you
13 don't have to be -- so that we don't have to
14 scribble, either. So you can look and
15 listen and be engaged.

16 MR. GRANATO: Ideally, we want you
17 just to make notes if someone's talking and
18 then interject something, just keep the
19 discussion going.

20 MR. SCIOLI: I note we've been
21 joined by two of our colleagues, and others
22 may come in. Greg Price from the Economics

1 Program, whom I hope you know; and Rachelle
2 Hollander, also from Social and Economic
3 Sciences. Say hello to them if you have any
4 business.

5 Okay, Jim has some opening
6 comments and then we'll get started on the
7 agenda.

8 MR. GRANATO: Thank you all for
9 participating in this workshop. To
10 paraphrase Admiral James Stockdale, "who are
11 you and why are you here?"

12 The answer to the first question
13 is simple: You constitute the very best
14 that political science and other disciplines
15 have to offer. Your scholarship
16 demonstrates a willingness to engage in work
17 that is innovative and that meets the very
18 highest of standards.

19 In short, the way you analyze
20 questions in your research makes you
21 uniquely suited to address the issues that
22 led to the creation of this workshop.

1 The answer to the second question,
2 why are you here, is a bit more complicated.
3 You are here because there's a growing sense
4 that political science has endured a
5 technical separation between formal and
6 empirical analysis for far too long.
7 Indeed, the separation serves as a barrier
8 to the scientific study of politics.

9 Now, what is meant by the
10 scientific study of politics? Well, among
11 other things, the scientific study of
12 politics requires building theoretically
13 informed models that take account of
14 confounding factors that may undermine our
15 inferences, or our betas; our predictions,
16 or y-hats; or conducting policy simulations
17 or some combination of all three.

18 Consider how a split between the
19 two approaches undermines progress. First
20 examine the risks associated with a strictly
21 empirical, read applied statistical
22 approach. Assume that the empiricist's

1 theory dictates the empirical model contains
2 more than one equation.

3 If one were to use a fairly
4 rigorous standard, then the empirical model
5 would need to be identified and thereby
6 satisfy order and rank conditions. But even
7 if a model is over- or just-identified and
8 the zero order restrictions are credible, it
9 is still possible the various parameter
10 magnitudes constitute a result that
11 undermine the entire theory; for example, an
12 indeterminacy in a model that says the
13 opposite.

14 Unfortunately, empiricists would
15 not know this, given their singular
16 approach. Instead, they note the model is
17 identified and would dutifully report the t-
18 and F-statistics, the size and sign of the
19 parameters, and believe they have created
20 something valid that advances our stock of
21 knowledge.

22 Yet this situation is not

1 necessarily long-lived, since any ex-ante or
2 conditional forecasts using these
3 "indeterminate" within sample parameter
4 magnitudes would be inaccurate, even
5 freakishly so.

6 There is also the distinct
7 possibility that the residuals created in
8 this estimation are not iid. Of course,
9 it's possible to hid this problem by
10 applying some residual weighting technique,
11 which can be done and, unfortunately, is
12 done. More on that later.

13 On the other hand, had the
14 empirical model been derived from a formal
15 model in a fairly straightforward way, it
16 would become clear that certain limiting
17 conditions of various parameter values
18 produced the inconsistency between theory
19 and outcome.

20 Now, consider a strictly formal
21 approach. Assume that the modeler devises
22 an elegant model that, after much work,

1 produces a single equation with a closed
2 form solution. She also determines that an
3 empirical test of the model with actual data
4 is in order. The model is linear in
5 parameters and functional form, so the
6 modeler chooses OLS. She runs the
7 regression, and sure enough, this
8 conscientious formal modeler finds the
9 residuals are not white noise. What is the
10 modeler to do? Well, the specification took
11 a good deal of effort -- months, maybe even
12 years -- to devise.

13 So to keep the specification, the
14 modeler weights the residual
15 variance-covariance matrix and applies GLS.
16 And voila! Residuals are now iid, and like
17 the empiricist above, the formal modeler
18 reports the t- and F-statistics and shows
19 that the hypotheses and theory are
20 supported.

21 Now, what's wrong with this
22 picture? First, the non-random behavior of

1 the residuals is a clear sign that the model
2 and theory are misspecified. No application
3 of GLS, no matter how powerful and efficient
4 the technique used to weight the residuals,
5 can cure this. Such practice is simply
6 incoherent, since it makes no sense to
7 correct the empirical model using
8 information created by the misspecification
9 in the first place.

10 More importantly, why would it
11 ever make sense to correct the model by
12 relying on the mistakes the model created?
13 The model is wrong. It's as simple as that.
14 In the end, this will be borne out again and
15 again by out-of-sample forecast failures,
16 both ex-ante and conditional. Nothing is
17 learned, nothing is gained. There is no
18 advancement.

19 As you can see, we have similar
20 outcomes starting with different approaches.
21 Are these examples exaggerated? Are they
22 caricatures? One need only to look at the

1 discipline's most selective "A" journals to
2 uncover the answer. The journals are
3 replete with empirical patchworks, such as
4 the weighting of residual
5 variance-covariance matrices, that attest to
6 the failure to portray accurately political
7 phenomena. Both approaches, acting
8 independently or carelessly borrowing from
9 each other, are equally guilty. These
10 practices are pernicious.

11 And this is why you are here.
12 With your help, the Political Science
13 Program seeks ways to take the lead in
14 ensuring that current practices that are a
15 consequence of this split become a thing of
16 the past. There are many ways for this to
17 happen, and indeed in some quarters, it is
18 already occurring.

19 But this is not just about
20 technique. Rather, technique is a vehicle
21 that, appropriately applied, can be used to
22 reach our ultimate goal -- a deeper

1 understanding of political phenomena.

2 A word about the excellent
3 commentaries is also in order. In their
4 discussion about the issues at hand,
5 workshop attendees noted the split is
6 natural. Others also noted similar patterns
7 in other disciplines. It should be said
8 that whatever the degree of pessimism or
9 optimism expressed in the commentaries,
10 certain themes do exist.

11 First, there is a problem with
12 current technical practice. Second is the
13 conviction that something can be done.
14 Third, NSF can assist in this exercise.

15 So how does the practicing
16 political scientist, the practicing social
17 scientist, who sees the utility of reducing
18 the divide, or is at least interested enough
19 to give it an honest attempt, alter the way
20 they currently practice their trade? A
21 better answer to this question is a central
22 issue on the agenda before us for the next

1 day and a half.

2 While it would be presumptuous to
3 think this issue will be resolved in this
4 workshop, progress can be made. Indeed, as
5 many of you have already noted, progress has
6 been made in recent years. These relatively
7 scarce works showing the link between theory
8 and empirics are found in unpublished
9 manuscripts, articles in various journals,
10 and conference papers. For the most part,
11 this research is motivated by a variety of
12 subfield-specific concerns. However, they
13 also contain a link between theory and
14 empirics suitable for much wider
15 applicability.

16 The hope of extending that
17 accessibility -- the implementations of the
18 workshop recommendations -- has not been
19 lost on those who are participating today.

20 MR. SCIOLI: Okay, with those
21 opening comments as backdrop, the first
22 discussion point, in which all, of course,

1 are invited to participate, but where we
2 have identified small groups to kick off the
3 discussion and please bear in mind that we
4 hope to do topic 1, identification of the
5 factors contributing to the split between
6 formal theory and empirical modeling in each
7 of the subfields of political science,
8 within an hour and a half. So we know that
9 you have many more insights than you can
10 share in that period of time.

11 We'll continue with that topic
12 after a break, but at the outset, let's
13 start with the American Government and
14 Politics subfield, which certainly by any
15 measure is the largest subfield of the
16 discipline. And John Aldrich, Bill Keech,
17 and Becky Morton will kick off that. So
18 John, if we're going alphabetically, would
19 you like to give us your thoughts?

20 MR. ALDRICH: Yes. First, I'm
21 simply here because, unlike many of you, I'm
22 often mistaken for Pollyanna. So I thought

1 I'd start off with some of the good news.

2 I was thinking about when, back
3 in -- just to show how far things have gone;
4 back in Rochester in graduate school in the
5 late '60s, early '70s, a couple of items:
6 One is that formal modeling hit a difficult
7 time finding enough readings to fill out the
8 second semester after his game theory
9 course. That's not so true today. One of
10 the really great advances has been just the
11 development of rational choice and its
12 diversity -- starting off within American,
13 but going beyond.

14 The second thing is that it was
15 quite common for people to be mistaken for
16 both theorists and methodologists
17 simultaneously. There was one semester I
18 took econometrics from Dick McKelvey and a
19 course where I learned behavioral decision
20 theory for the first time from Chris Achen.
21 And it could be that -- I mean, people have
22 pointed out that my theory looks like it was

1 taught by a methodologist and my methods --

2 (Laughter)

3 I think that represents something
4 about how far apart the whole discipline was
5 from anything like a regression equation or
6 a 2-by-2 game of prisoner's dilemma at the
7 time. And all look alike. Indeed, when I
8 got to Michigan State no one was ever sure
9 what position I -- my first job, no one was
10 sure what position I had, including the
11 chair and myself.

12 So the two great accomplishments
13 have been in the development of the
14 possibility of having a split. Thirty years
15 ago we couldn't really have a split because
16 there wasn't anything to split.

17 Second point is that it seems to
18 me that if there's going to be some
19 progress, it's more likely, just in my view,
20 to come from changes in the way political
21 methodology is done than in the way theory
22 is done. And the reason I think that is

1 because, first of all, it's very difficult
2 to imagine, you know, Mickey Rooney playing
3 Andy Hardy saying, "Hey, let's go back in
4 the barn and build a new theory." It
5 doesn't happen quite that way, whereas
6 methodology has more of a problem-solving
7 structure to it. And therefore the choice
8 of problems is somewhat easier to move,
9 marginally.

10 Secondly, it seems to me the thing
11 that should be changed in terms of political
12 methods is -- at the current time, it seems
13 to me that there's an overconcern with
14 developing estimators and underdevelopment
15 in the full range of all the stuff that,
16 kind of, — methods.

17 I know there are some -- several
18 of the memos talked about qualification and
19 this and that and the other, and a lot of
20 things that should be done under methods,
21 testing of theory doesn't necessarily
22 require elaborate statistical estimation in

1 all circumstances -- either — understand
2 it or they didn't need to estimate that.

3 And the third thing that I'd like
4 to see some attention to and I think Becky
5 mentioned this, is that we do a really bad
6 job of recruiting undergraduates to go to
7 graduate school. I don't know about most of
8 you around this table, actually — than
9 in most cases -- but most people don't
10 become political science majors to study the
11 actual science of politics. They're just in
12 public policy, in government, law school,
13 whatever. And we don't either attract --
14 efforts to attract people who might be
15 scientifically interested in the study of
16 politics or even let undergraduates know
17 what's involved in it very often, that there
18 is such a thing. Maybe Becky's experience
19 at Iowa could be a little bit of use.

20 It seemed to me that if we had
21 something like a double major, a government
22 and politics major, a B.S. in political

1 science, that -- where we would actually go
2 to undergraduates and teach them, not unlike
3 economics teaches real or early versions of
4 economics to economists, that we would do a
5 much better job of recruiting people into
6 graduate school and be a signal that, out
7 there to undergraduates, there is such a
8 thing as political science, and we mean it
9 seriously and just as a label to cover up an
10 easy way into law school.

11 And the final thing is, if we are
12 ever successful at this, we shouldn't have
13 the current state of the subfields being
14 American Government, Comparative, and
15 International. They should be devised
16 problems sets by problem, micro or macro. I
17 mean, Becky's idea of -- I think it was
18 yours, wasn't it, of International and
19 Domestic? Bob Coho and I have been talking
20 about changing our graduate program along
21 those lines and just doing away with the
22 American-Comparative distinction and

1 splitting Comparative in half.

2 Those are things that occurred to
3 me that I didn't put in my memo.

4 MR. SCIOLI: Bill Keech.

5 MR. KEECH: Well, with respect to
6 the question of the actors contributing to
7 the split, I think it's to be understood
8 historically in political science, and I
9 think the history of the split is different
10 in political science than it is in
11 economics. In my memo I gave a quick sketch
12 of this. Let me just be even quicker in
13 repeating the nature of that as I saw it.

14 I think that the scientific study
15 of politics began as a quantitative study
16 that was empirical and basically inductive,
17 and theory in political science has meant,
18 at least in the past, a political philosophy
19 and the history of ideas. And in the '60s I
20 don't think there was much sense of theory
21 in political science, but there was an
22 excitement about science and quantification.

1 And it was largely inductive, based on --
2 started on the study of voting behavior.
3 And arrived first at Michigan and there was
4 a kind of missionary impulse coming out of
5 the ISR at Michigan.

6 I think theory came along later.
7 It was largely an import from economics.
8 And I think Anthony Downs and Mancur Olson
9 had a lot to do with that. And I think
10 theory came in in the guise of rational
11 choice models. And so there has been a
12 split generated by the fact that there were
13 two independent sources of these two
14 scientific movements.

15 Now I would like to think that
16 historically determined is not equivalent to
17 past-dependent. And I think that it is
18 possible to break out of this. It makes me
19 think about economics -- I think for people
20 who didn't come in with us, they're not
21 aware of this history. And for them, the
22 difference between theory and empirical work

1 is more like that in economics, where I
2 don't see the kind of hostility that there
3 is in -- which I hope will be a thing of the
4 past -- in political science.

5 I think that the main source of
6 theory in political science has been
7 rational choice models. And rational choice
8 is a very unfortunately divisive term in
9 political science. One thing I think would
10 help make progress is, and I'm not sure how
11 this is going to happen, but for there to be
12 greater sympathy for modeling on the part of
13 political scientists. And I think this may
14 need to involve some loosening on both
15 sides.

16 Rational choice models are, in one
17 respect, simply models that give human
18 beings the credit for being intentional
19 thinking beings. That should hardly be
20 controversial. And at the other extreme,
21 there's all kinds of reasons to think that
22 human beings are not like the classic

1 rational choice actors with perfect
2 foresight and infinite computing capacities.

3 There's a lot of work being done,
4 much sponsored by NSF, about behavioral
5 economics and how human beings are really
6 slow and inefficient information processors
7 and often have systematic deviations from
8 rationality.

9 I think political science would be
10 a lot better off this kind of idea was
11 associated with the conventional view of
12 modeling and theory, and that might overcome
13 some of the hostility to theory which in
14 political science, I think, is partly driven
15 by the high entry costs of mathematical
16 sophistication, which I think are higher
17 than they are for empirical work, and partly
18 what I consider a pretty irrational
19 hostility to modeling in this rational
20 choice tradition.

21 I think progress is being made,
22 but I think -- I'm discouraged by the amount

1 of resistance to this. I'm less discouraged
2 now because I'm not in an environment of an
3 ordinary political science department. But
4 it's never ceased to amaze me how much
5 hostility modeling generates among otherwise
6 intelligent people in political science.

7 Let me endorse and echo the
8 comments that American politics is an
9 artificial subfield. It's the most arrogant
10 of the area studies and the idea that
11 American politics is unique is something
12 that's certainly true, but this uniqueness
13 is better understood in a comparative
14 framework. And I'd just like to endorse the
15 idea that there be domestic politics.

16 I've suggested in my memo that
17 John Londregen is studying American
18 politics, and American politics includes
19 South and North and his book is a marvelous
20 combination of empirical work and
21 theoretical work. And in that sense, I'd
22 like to think that American politics, the

1 study of American politics can go forward.

2 But it's not just Western
3 Hemisphere politics, it's a study of
4 domestic political processes, wherever the
5 location.

6 I once said something that I wish
7 I hadn't said -- and I'll say it again
8 because I think this is a friendly audience.
9 In another context I -- what were cultural,
10 the residual explanation for things that you
11 couldn't put in models, I said culture is
12 not a variable.

13 Actually, the more I think about
14 it, the more I have to recognize that
15 culture is a variable and it's a pretty
16 important variable. And one thing I'd like
17 to see is a more scientific way of measuring
18 and incorporating culture as a variable
19 rather than as a residual explanation for
20 things that can't be explained otherwise.

21 MR. SCIOLI: Becky Morton.

22 MS. MORTON: These guys didn't

1 leave me very much. You know, I basically
2 see the reason for the divide pretty much
3 the same as Bill and John have talked about.
4 I mean, empirical work developed sooner, has
5 a longer tradition. Theory's been playing
6 catch-up. I think that's a big issue.

7 I also think that one of the
8 reasons why empirical work has a better hold
9 is -- you know, you come into graduate
10 school, these students typically have very
11 little math backgrounds, if any -- I mean,
12 many haven't taken any math since high
13 school. If they had calculus in high
14 school, they didn't have to take it in
15 college and so they don't really know any
16 math.

17 And they come in and, well, you
18 know, there are these empirical software
19 programs where you can do some very complex
20 things, and you still don't ever need to
21 know any math. And we, unfortunately, have
22 a lot of empirical people out there in

1 political science who really don't know what
2 they're doing.

3 So I think that that's a serious
4 problem, because they just come in and they
5 learn how to start up, put the data in, and
6 they run regressions, and they really don't
7 have any clue of what they're doing. And I
8 think that there are lots and lots of
9 programs like that. And I've seen it at
10 Iowa and I've seen it at Houston -- even at
11 Iowa, where we had required math class, they
12 end one semester and it just seemed to go
13 out their ears and they still didn't know
14 what they were doing.

15 Whereas when you do theory, you
16 actually do kind of have to know what you're
17 doing, because there are these referees.
18 And the standards I see often for theory in
19 political science are extremely high. So I
20 think that it's much harder to do theory.
21 And I think that the students come in, they
22 look down the road, and they want to get

1 jobs, and they want to do something that can
2 get published, and there's a big emphasis in
3 our graduate programs in getting articles
4 out and getting out in 4 or 5 years, while
5 there is a much easier route if you just use
6 that empirical side.

7 Maybe you might take a little of
8 that formal theory, but only the light
9 stuff. They don't want to really take the
10 difficult stuff because that's just way too
11 much of an investment.

12 And there's got to be some change
13 in that. I mean, I think that that -- as
14 long as that continues -- I mean, eventually
15 I think these things will correct
16 themselves, as I think Chris sort of hinted.
17 But it needs a push. I mean, it's just
18 going to take a long time if we just let it
19 go on and on as it is.

20 I think part of it is because our
21 students have these other motives when they
22 come to undergraduate school and they take

1 political science courses, and now I'm in
2 the department of politics, you know, this
3 is, like, really scary. But at least I'm
4 not in government -- but anyway, at NYU it's
5 the Department of Politics, not even
6 political science.

7 So I really think that, you know,
8 this fact that the empirical work developed
9 sooner and then we had these canned programs
10 that allow people to do empirical work
11 without having to really know any math.

12 I also think that a big source of
13 the problem is this anti-rational choice
14 thing. I think it kind of goes both
15 sides -- I mean, I think that when I read
16 some of the stuff in political science about
17 rational choice, it's much more defensive
18 and, you know, out there than some of the
19 stuff I read in economics. And I think that
20 that's partly because rational choice
21 modelers in political science had to fight
22 such a big battle just to be heard. And I

1 think that, you know, there might be some
2 need to lighten up a little bit.

3 And then some of the early
4 conceptions of the way rational choice was
5 explained to political scientists emphasized
6 the self-interest nature, it emphasized
7 certain things that, you know, political
8 scientists really found offensive, and many
9 political scientists don't know that
10 things -- they haven't read beyond that. I
11 mean, there's, you know, a lot of what is
12 taught in the political -- you know,
13 introduction to research design classes
14 about what theory is is very old stuff, you
15 know, like Downes. And they read the whole
16 Downes and they spend a lot of time talking
17 about it, and they don't read some of the
18 more, you know, stuff published in the last
19 40 years.

20 This is not a good way to
21 introduce students to the current state of
22 formal theory, because they're seeing this

1 really old stuff that has a very particular
2 rational-choice bent that they find
3 offensive. And they find the other more
4 psychological stuff they're getting taught,
5 that maybe doesn't have any formal theory
6 context, more appealing. We really talked
7 about the math part.

8 Again, you know, I basically think
9 that American politics -- what needs to
10 happen is we need to stop thinking about
11 American politics as American politics, as
12 these guys have already said. I do think in
13 American politics, though -- or, I guess, in
14 domestic politics -- I shouldn't use this
15 term, then -- there is this division between
16 using formal theory for looking at elite
17 stuff, like legislatures and executives, and
18 talking about voter behavior.

19 There's a lot of theories out
20 there about elections and voting behavior,
21 but there really is an unwillingness to
22 accept that work. And when we get to

1 voting, in an individual-level analysis in
2 political science, we tend to drop all the
3 theory, as far as I can see. I mean,
4 there's some theory, but it's not making the
5 same headway as it is studying Congress.

6 And I really think, again, this is
7 because of an unwillingness to think about
8 voters as decision-makers and the
9 attractiveness of the Michigan voter model
10 of what's going on in elections.

11 So I see that area as probably the
12 weakest area in domestic politics, in terms
13 of theory. There's lots of the empirical
14 work out there, but there doesn't seem to be
15 the connection to theory that I would like
16 to see.

17 So I guess that's about --

18 MR. SCIOLI: Would anyone else
19 like to weigh in on this subfield of
20 American politics that we've just
21 eliminated?

22 (Laughter)

1 SPEAKER: Could I just ask a
2 question? Do the Americanists agree that
3 Congress studies are the most advanced and
4 that they come closest to meeting the ideal
5 that Jim laid out?

6 MS. MORTON: Oh. Actually, I
7 think the theory's advanced. I think
8 there's a lot to be said -- needed for the
9 empirical work in the sense -- and I think
10 that's why we need to move beyond thinking
11 about Congress, and we need to think about
12 legislatures, because I think too much of
13 the theory is explaining how this particular
14 rule in Congress works.

15 And then when we can't really do
16 this comparative analysis because -- except
17 maybe over time, and then there's all kinds
18 of problems, right, and so we have to do it
19 across legislatures. So --

20 MR. ALT: If I could just step in
21 there. I think it isn't even a matter, in
22 my eyes of just saying we want to go from

1 Congress to legislatures. Legislatures are
2 still too big, you want to go, I think, down
3 a level to stylized problems and the
4 mechanics for solving them -- delegation,
5 bicameralism -- you know, things that are
6 institutional features of legislatures. And
7 then by all means study America if you do
8 bicameralism -- kind of like — along with
9 Germany and other places that have -- you
10 discover that bicameralism itself varies in
11 interesting ways once you're looking at it.

12 As I said in the first paragraph
13 of my memo, if you really want to study
14 America's institutions, a great way to do
15 comparative politics is to study the states,
16 because you have about 47 replications of
17 industrial societies with, you know, open
18 economies that have these bicameral
19 legislatures and a separate executive -- and
20 that's data.

21 So, you know, it seems to me part
22 of the answer to this conundrum is that

1 American Politics should become Comparative
2 Politics by using the states as data much
3 more than has been the case in the past,
4 even though, in the common spirit of all
5 these memos, I'm happy to say that good
6 things are happening. You know, there's an
7 APSA short course on state data sources this
8 year, and that's exactly how the
9 infrastructure gets going, it builds up
10 these possibilities.

11 And in the same way, I'd like to
12 see comparativists tackle, along with many
13 other things, the kinds of institutional
14 mechanics, things like delegation or
15 bicameralism or whatever, what I think are
16 the stuff of which a real integrative
17 comparative political science would be made.

18 MR. ALDRICH: I think one of the
19 reasons that the modeling part of
20 legislative politics seems so advanced is
21 because it seems to be addressing things
22 that the rest of legislative politics

1 scholars find substantively interesting --
2 the structure of Congress, the stuff that
3 people who don't do rational choice find
4 interesting. So there's this sort of
5 sympathy that's been a sort of positive
6 development for the encouragement of what
7 kinds of problems and how it developed over
8 the last 15, 20 years.

9 MR. KEECH: One thing that the
10 study of legislatures -- or to address it as
11 Congress, referring to the U.S. -- but I
12 have said that I think the study of Congress
13 is the most advanced, and thereby agree with
14 you. One thing that makes this true is that
15 it's received so much attention and there's
16 so much manpower and NSF money and so on
17 behind the study.

18 There's something natural about
19 the setting, though, that I think makes it
20 have a lot of potential. This is an area in
21 which preferences of constituencies and the
22 attitudes of voters and citizens are

1 systematically brought to bear on public
2 policy and law making. And there's the
3 study of individual behavior, there's the
4 study of legislative behavior, but one thing
5 that makes Congress particularly challenging
6 and, if we're going to expand our language,
7 legislatures particularly challenging and
8 uniquely political -- that is, relevant to
9 political science -- is that, unlike other
10 disciplines, this is a place -- oh, it's not
11 a -- let me -- scratch that.

12 The relationship between what
13 public wants and what governments do is,
14 really, central to the study of politics.
15 And that's one reason that -- I mean, it's
16 not just the amount of attention and money
17 that's been put into the study of Congress,
18 but it's that feature of bringing together
19 publics and policy making that makes it
20 uniquely part of the subject matter of
21 political science and it has a lot of
22 potential for advancement that has been

1 fulfilled, I think, in the study in the
2 past.

3 MR. ALDRICH: A second problem is
4 that -- a second dimension of this is that
5 not only is it where people actually, you
6 know, visibly strategize -- that's nice for
7 applying strategic modeling to -- but it's
8 also, looking at it as uniquely political in
9 addition, is it's one of the places where we
10 can observe how they're -- you know, rule
11 makers as well as policy makers, and how to
12 control -- might want to control the rule
13 makers is a frustrating problem of politics
14 as well.

15 MR. SCIOLI: So is it the sense,
16 then, that this is where the greatest gains
17 have been in the linkage between formal and
18 empirical -- the legislative American
19 politics sub-subfield?

20 MR. ACHEN: It's been a lot of
21 work. Not precisely the same thing.

22 MS. ZINNES: Lots of articles.

1 MR. ALT: I mean, if you meant
2 within American politics -- within the study
3 of American politics, I'd probably find it
4 easier to say yes than, you know, across the
5 field of political science as a whole.

6 MR. SIMON: Maybe this discussion
7 points out to one of the difficulties in
8 developing the political science theory. In
9 economics, one would never talk about which
10 subfield is the most natural for developing
11 a theory. But the fact that there are so
12 many subfields, or a number of subfields,
13 and that the approach to each can be very
14 different I think is possibly one of the
15 barriers to doing the kinds of things we're
16 talking about.

17 MR. ACHEN: I actually think that
18 I may disagree with Becky a little bit. I
19 actually think there has been a pretty
20 substantial influence of former models on
21 the voting research side. And I've sent
22 Becky references and so forth hoping some of

1 them would get into the book that she did
2 with Chuck, and so far I haven't been very
3 persuasive.

4 MS. MORTON: Well, let me just say
5 that's Chuck's fault.

6 MR. ACHEN: I never doubted that.

7 MS. MORTON: And he knows that I'm
8 saying that.

9 MR. ACHEN: But, you know, part of
10 the difficulty, of course, is that an
11 individual voter is not a strategic actor in
12 any important sense -- you can't influence
13 what anybody else does on your own. So it's
14 mostly, you know, decision theoretic rather
15 than game theoretic, but the Bayesian models
16 of learning, for example, and how voters
17 update, and differences in information or
18 class voters, as well as some recent work on
19 turnout that's been explicitly timed, the
20 theoretical model, to the econometric stuff.
21 All that's been done with the individual
22 voting data.

1 And it kind of seems to me it's
2 been quite interesting and, by my lights,
3 quite closely tied to the empirical evidence
4 in ways that is much harder to do with
5 legislators, where you've got so many fewer
6 observation points.

7 So I guess I'm not convinced that
8 the voting field is bereft of this
9 connection. In fact, it seems to me that
10 some of the best opportunities for tying
11 theory and data lie exactly there and are
12 being exploited.

13 MS. MORTON: I guess I have to
14 admit that I think that my view of the field
15 is covered by referee reports and things
16 like that.

17 MR. ACHEN: That'll cover
18 anybody's --

19 MS. MORTON: And since I work in
20 it, I see the problems in it more bothersome
21 than, you know -- so, you're right. There
22 is lots of good work out there in different

1 elections.

2 I should add also, we haven't
3 talked about experiments -- you know,
4 Richard mentioned that and Jim did. And
5 certainly in terms of domestic politics of
6 looking at a lot of the former models have
7 been developed for American, these things
8 have also had a big history in terms of
9 experimental work. And experimental
10 research that's formal theory based is very
11 much a testing theory. And I think that
12 that's a really important part of the
13 research of domestic in votings of elections
14 in particular. And Cheryl certainly -- her
15 work on that.

16 MR. BRADY: I think one of the
17 problems with voting behavior, too, is that
18 we draw a lot upon psychological research.
19 And if anybody spends much time reading
20 psychological research and one darned
21 experiment after another, you start -- at
22 least I start screaming after awhile that I

1 wish there was a theory that put some of
2 these variables together.

3 Because it -- everybody's got
4 their little variable they've done
5 experiments on -- yes, looks like there's
6 some evidence, that has an impact; then
7 another variable, another variable. And you
8 just want somebody to sit down in psychology
9 and say can you put some of these together
10 into some kind of elegant formulation.

11 Now, maybe we're going to get that
12 from the behavioral economics direction.
13 They may be the people who actually start to
14 do that, which would be enormously helpful,
15 I think, to voting researchers.

16 MR. ALDRICH: Why isn't it us who
17 are doing this? Why were you -- were you
18 looking for a psychologist to do it? Isn't
19 it our job?

20 MR. BRADY: Yes.

21 MR. SIMON: One advantage voting
22 has over almost any other subfield in any

1 area is the clear choice; I mean the
2 distinct sharpness of the outcome. I mean,
3 economists have price, but what's the price
4 of a car? Are you going to -- of a Dodge or
5 van. But votes, Florida notwithstanding,
6 are the sharpest measure we have in almost
7 any science, including physics, of an
8 outcome.

9 MR. SCIOLI: I don't think we've
10 ever heard it characterized that way, but
11 I'll not forget your statement.

12 MR. SIMON: I'll fight to defend
13 it.

14 MR. YOUNG: Can I jump in with a
15 question to Chris? What -- although I'm
16 perfectly willing to accept Bayesian models
17 and this, that, and the other, but I mean
18 you expressed some enthusiasm for their
19 ability to possibly explain certain futures
20 that turn out. The trouble in my experience
21 with those kinds of papers, though, is it's
22 very rare to actually consider an

1 alternative model that would do just as
2 well.

3 In other words, a typical kind of
4 paper that says, well, now, here's a rather
5 fancy, high-end sort of model, I'll say
6 based on basing updating, just to take an
7 instance, that explains the fact, which may
8 be quite well-supported in data. But then
9 there's no attempt to say, well, but
10 actually here are one or two other theories
11 lying around that could, or might not,
12 explain the same data; possibly even more
13 parsimonious theories.

14 All I'm saying is that that is not
15 a typical way you see a paper written. And
16 I think that this is related to our quest
17 here, although I'm not quite prepared to say
18 how one changes the standard, so to speak.

19 MR. ACHEN: Well, as you know from
20 the paper I wrote, I'm a skeptic. I talked
21 explicitly about Bayesian models of voters
22 and the problems that they have with data.

1 So I think my position's pretty explicit on
2 this.

3 Nevertheless, as Henry just said,
4 the voting literature comes out of social
5 psychology, for the most part. And there's
6 a ton of empirical generalizations. One of
7 the nice features of the Bayesian framework
8 is that you can generate those empirical
9 generalizations just by turning the crank,
10 proven theorems. A lot of people have done
11 that.

12 Now, you might argue some other
13 framework could also derive these 12
14 propositions. Nobody's done that. I guess
15 I think that the burden of proof is on the
16 people working in the other frameworks.

17 MR. SCIOLI: The subfield
18 designations that we've made have certainly
19 been artificial, as Jim Alt has claimed, no
20 longer being a comparativist and as we
21 pigeon-hole John Freeman. But this might
22 segue into another subfield.

1 And I guess my experience has been
2 that in terms of comparative government and
3 comparative politics, we've tried many
4 things at NSF to try to jump-start the
5 comparative politics subfield and to try to
6 move it away from, I guess, the way we were
7 all trained, except for Jim, in graduate
8 school looking at area studies.

9 Can you share with us your view on
10 how this field is progressing?

11 MR. ALT: Well, I said most of
12 what I had to say in the memo. Let me just
13 hit the highlights. Let me start -- I do
14 want to start, for a second, because there
15 is a sort of underlying theme here. There
16 are a couple that are sort of floating
17 around this discussion, but they're worth
18 articulating.

19 The first is that I think this
20 conference is a very good idea -- a very
21 good idea, I think as all the memos made
22 clear, not because there's such a huge

1 problem in the field. In fact, as most of
2 the writers said, this is a problem of
3 science, not a problem of political science.
4 And in fact in some ways it's just a
5 reflection of specialization and cumulation
6 of results and things like that.

7 And so I didn't want to echo what
8 John Aldrich said in his opening sentences.
9 When I was a kid breaking into this field,
10 my heroes were people like Dick McKelvey,
11 Gerry Kramer, Norman Schofield, who were
12 innovators both in theory and in methods.
13 And that was a very good thing for me
14 because I knew right away that I didn't have
15 their abilities and I'd better find an
16 easier way to make a living. And I
17 discovered computing and empirical work and
18 never looked back.

19 So that was, sort of, my first
20 personal reflection, you know -- some of the
21 things we want people to do, people don't do
22 in individual pieces of research because

1 it's really, really hard and you have to be
2 really, really smart to do it. But that
3 doesn't mean you shouldn't ask, right?
4 Because that's the goal of science, to
5 cumulate by individual efforts what would be
6 beyond all but, you know, maybe everybody,
7 or the very, very best to do on their own.

8 When I think about the way I work,
9 this is just the way I set out the memo. I
10 figured what I could contribute here, maybe,
11 were some thoughts on what's special about
12 comparative.

13 So I thought a little bit about
14 how I work and that's why I had those kind
15 of, you know, five steps to a paper in
16 there. You have a puzzle, you have verbal
17 theory, you have a formalization, you have a
18 notion of a test, and you have data.

19 And actually, I don't have
20 anything very deep to say about that, except
21 that it was fun to think about those steps
22 because in many ways a lot of the problems

1 that we have in comparative come from
2 different ones of those steps being dropped
3 out of the enterprise.

4 The first one that I sort of
5 dismiss is still the core problem -- used to
6 be the core problem: That's a great idea,
7 but how on earth are you going to get the
8 data? The data, you know, was the principal
9 constraint. And I'm happy to say that a lot
10 of NSF money, a lot of individual effort,
11 and a lot of development of techniques for
12 imputation to solve the missing-data
13 problem.

14 On the whole, that is not where I
15 would start the discussion in comparative
16 politics. Other things are interesting,
17 like people who leap from verbal theory to
18 testing without going down the road of
19 formalization in-between. And there I do
20 have something to say, so I'll get there.

21 But it seems to me, first and
22 foremost, if you think about it the way I

1 said, the puzzle that comparativists start
2 with, by contrast to the rest of the field,
3 is always a very macro puzzle. They think
4 about big things, like revolutions. Bill
5 Riker spent the crotchety part of his life
6 saying, "Why don't they just think about
7 smaller things?" And he never really had
8 any success getting them to do that, so I
9 don't think we will, either.

10 But I think, you know, that's sort
11 of the first thing to think about, is that
12 even in the institutional literature, most
13 institutions are pretty big. If we're
14 talking about countries over epochs, we are
15 not talking about actors that have the
16 beliefs and preferences of individuals that
17 characterize rational models.

18 And so dealing with the
19 aggregation problem -- which, interestingly,
20 was actually the core of Jim's and Frank's
21 original proposal and, maybe surprisingly,
22 wasn't picked up that much by people writing

1 for the workshop -- but that is a real, real
2 problem. I think the best way to see how
3 real a problem it is is to look at, say,
4 someone like Avner Greif, who would be a
5 contender for the successor to my early
6 heroes award, who has spent 10 or 12 years
7 trying to reason out a model for what is in
8 fact a very simple institution, the Podesta
9 in medieval Genoa or wherever it was.

10 I call it a simple institution
11 because the Podesta was only one person.
12 There were only two families in conflict
13 that he was supposed to keep the peace
14 between. And he was only allowed to have 20
15 soldiers, no committees, and he didn't need
16 an affirmative vote of a majority of his
17 soldiers before taking action.

18 So really you would think this
19 would be an institution you could reason
20 about, and Grief set himself the challenge
21 of deriving this institution as the
22 equilibrium solution to a particular

1 problem. And the last time I saw a draft,
2 12 years and some 500 manuscript pages
3 later, he's not close.

4 So never underestimate the
5 difficulty of these aggregation problems in
6 getting to the equilibrium analysis of
7 institutions -- would be kind of the main
8 theme, what's the problem for comparative?
9 It starts by looking at big things and it
10 has this characteristic problem.

11 The problem we've all heard much
12 more about is that when you turn from the
13 puzzle to the verbal theory, there's no
14 agreement whatsoever in the field about what
15 the verbal theory is about, and that's why I
16 love citing that book by Lichbach and
17 Zuckerman, Rationality, Culture, Structure.
18 That's it. That's all you need to know, the
19 title of that book. What should our
20 dependent variables be? Well, rationality,
21 culture, structure, and you're rolling. So
22 there's a book that's much more than a table

1 leveler and I think they're right.

2 I think the line for the sermon
3 part of this I didn't think of when I wrote
4 the memo, but it occurred to me,
5 particularly because many people drew
6 comparisons with economics, to talk as I do
7 about a puzzle and verbal theory in
8 political science. I have one, you know,
9 where is the theoretical hook to hang this
10 puzzle on -- is exactly the thing economists
11 finesse by presenting an intuition and a
12 stylized fact.

13 Far more economics articles begin
14 with a statement that something is a
15 stylized fact and presenting an intuition of
16 a model which addresses that fact than you
17 ever see in political science. And it's
18 good, because the articles that do that in
19 economics are the only ones, typically, that
20 I can understand.

21 And so I think -- I believe, Bill,
22 this speaks to your "importance" point, you

1 know, as well, because the stylized fact is
2 the presentation of the puzzle. That kind
3 of helps you get it right. It's important.
4 And the intuition of the theory is what
5 helps the audience get to, you know, the
6 relevance to solving the puzzle. And so I
7 don't know how we do that in political
8 science, but boy, we should have courses on
9 presenting the intuition and the stylized
10 fact in order to motivate the reader to get
11 through the rest of the paper.

12 Having said that, when we turn to
13 testing, this stuff is familiar, the debate
14 between empiricism and understanding the
15 debate between comparative and area studies.
16 I have got nothing new to say about that.

17 When people ask me about area
18 studies, I -- you know, to address Frank's
19 question -- I just tell them it's up to you.
20 I ask them what they're interested in and if
21 geography appears in their answers, as far
22 as I'm concerned they're area studies

1 scholars. And if no words that describe
2 geography appear in their answers, then
3 they're not. I think it's that simple and
4 we can leave it there.

5 I think there are lots of problems
6 around formalization in comparative. I
7 mentioned three characteristic things that
8 you hear a lot. One is what was there in
9 the model that couldn't have been said in
10 words? All too often the answer is
11 "nothing," because in fact the model was an
12 intuition followed by enough notation to
13 make you believe it was a formal model, but
14 in actual fact it wasn't.

15 Another thing you hear a lot, and
16 we've talked about it, is that the models
17 are all about features of American
18 institutions and they somehow don't fit
19 comparative. I don't think that's true at
20 all. I think even a quick look at the works
21 of Cox, Huber, Tsebelis, Londregan, who's
22 been mentioned a bunch, Laver and Shepsly,

1 mentioned in another -- makes it clear that
2 what's happening is that people are at most
3 taking a feature of American institutions --
4 something which is an interesting problem
5 better understood than some others, in the
6 case of American politics -- and taking it
7 off to other appropriate contexts.

8 I think there is, you know, not
9 just the books, but there are lots of
10 articles by Myerson and Diermeier and people
11 like that that make me believe that the
12 enterprise is really going well.

13 In terms of where is it going best
14 in comparative, to give you your example, we
15 can debate in parallel to the Congress
16 literature in American -- I would look at
17 the coalition duration, cabinet termination
18 literature.

19 Fifteen years ago it was nothing.
20 It was a couple of conjectures. Then some
21 people conjectured that the conjectures
22 weren't incompatible and somewhat

1 generalized the model and provided some
2 supporting data.

3 You see exactly in this scientific
4 enterprise data running ahead of theory. We
5 had intuitions, but the first people to set
6 down a good bargaining theory that predicted
7 the end of a cabinet were Lupia and Strom.
8 Count me among those who think there are
9 problems with that paper, but it's
10 nevertheless very interesting and
11 productive.

12 And then, of course, Diermeier and
13 Stevenson do what you want them to do, they
14 pick up that model and they actually derive
15 a statistical model appropriate to the
16 bargaining theory, and test it and show that
17 the theory mostly doesn't work but sometimes
18 does in some interesting ways. And I'm sure
19 we'll move on from here.

20 That, to me, is a perfect example
21 of the way in which a literature should
22 develop. You know, and we should all be

1 very happy that it is possible to do that in
2 comparative politics. It's not perfect, but
3 it's way ahead of where it was 15 years ago.
4 And it's pretty clear what the weak points
5 are and what the debates and arguments are,
6 and that -- when I say it's pretty clear,
7 it's pretty clear to theorists, on the one
8 hand, that the question is where are the
9 voters; and it's pretty clear to empiricists
10 that the problems with the data are whatever
11 they are. I don't participate in that
12 literature anymore, so I don't know what the
13 current wave of data problems are.

14 So anyway, it seems to me we're
15 progressing very well. Another thing that's
16 characteristic of comparative is just,
17 though, that there are so many topics that
18 people want to talk about -- failed
19 democracy, absence of the rule of law,
20 ethnic political strife. I listed half a
21 dozen off the top of my head in the middle
22 of the, I guess it's the third page of the

1 memo. And, you know, we just need modelers
2 and theorists to turn more to some of these.

3 What can we do better? Well, I
4 make two big and a small pitch at the end.
5 I think the biggest thing we can do better
6 is to do a much better job, and here I
7 really stand with Dick, on getting
8 experimental methods into the curriculum.
9 And I say this not just because, you know,
10 experiments have something unique to
11 contribute, but because right now there are
12 such unbelievable innovations,
13 methodological innovations, taking place in
14 the mechanics of experimentation that
15 comparativists, particularly, need to get
16 into this.

17 What I mean by that is Dick has a
18 pitch in his memo for lab experiments. And
19 what I observe around me now is that lab
20 experiments have left the lab. That is to
21 say, the lab is Rick Wilson's microcomputer
22 heading off to any context you want and

1 addressing for the first time, I think, in a
2 really good way the external validity
3 problem of labs with college students, by
4 actually not being either in a lab or
5 dealing with college students when he does
6 his experiments. This kind of innovation is
7 something that our people need to see as a
8 useful tool. And so we're trying to push
9 that.

10 The other thing I think, and
11 others said this as well, is the
12 post-doctoral fellows are just enormously
13 important. I don't understand, coming out
14 of comparative with its area studies people,
15 why every area scholar says, thinks it is
16 perfectly natural that a PhD does not equip
17 you to teach, it should be backed by a year
18 or two in a foreign country, learning
19 another culture.

20 Why the hell don't we say the same
21 thing? A PhD doesn't make you a professor,
22 it makes you ready to go and learn modeling

1 by doing, you know, on someone's research
2 project with someone who is good at taking a
3 model out of a theory and finding data and
4 making a statistical model that addresses
5 the theoretical model.

6 I mean, I believe we all believe
7 we have people who are capable of doing
8 that, who young PhDs could watch do it,
9 participate in doing it, and learn something
10 valuable in the process. And I just think
11 we need to reorient our notion of training
12 to include that. And I reiterate the point
13 that that's such a natural way for area
14 scholars, you know, to think. And I think
15 even in comparative we'd probably be able to
16 get their support.

17 So, that's my speech.

18 MR. SCIOLI: John Freeman.

19 MR. FREEMAN: I'll try to be
20 brief. In terms of a -- I guess I'd
21 characterize this as a metasplit, and try to
22 fix ideas in the way that Carl — in

1 particular, and what is this all about. I
2 think John encountered this first-hand last
3 year at the APSA planning committee with the
4 attack on science and political science.

5 There's a whole group in
6 comparative politics -- I'm not sure I'd
7 call them area studies people, I think
8 they're people who have at best a different
9 conception of science, and at worst are
10 anti-scientific.

11 So, Carl, when you talk about the
12 difficulty of our concepts, they would argue
13 that there is no cross-culturally meaningful
14 conception of democracy. There might be a
15 cross-culturally meaningful conception of
16 velocity, but not democracy. And Lynch
17 wrote a book 25, 30 years ago, right, the
18 idea of social science is denied the
19 possibility of cross-culturally meaningful
20 concepts like democracy and this is a
21 barrier to science.

22 Peyton would argue that the

1 heterodoxy of political science, its rich
2 debates, its epistemological controversies
3 are wellsprings of greater ideas, perhaps.
4 Kant never was, to my knowledge, invited to
5 a political methodology group meeting, but
6 he somehow, in his imagination -- in his
7 imagination, 300 years ago, conjectured that
8 democracies would not quite work. I mean,
9 this is the remarkable thing.

10 So maybe the heterodoxy is a
11 really wonderful, wonderful thing that
12 describes comparative politics. But it
13 seems to me the MacArthur people and others
14 have taken it to an extreme where it becomes
15 a barrier to progress in some meaningful
16 way, and I just want to reiterate that and
17 vent my spleen to some degree about it.

18 All right, so the split at hand,
19 not the metasplit but the split at hand,
20 three or four things that are problematic.
21 I think Jim's right -- a third kind of
22 training. When you give a presentation

1 amongst dyed-in-the-wool comparativists, the
2 first question they ask, well, how long were
3 you in the country. They don't ask if you
4 know the calculus of variations or whether
5 you know anything about Bayesian
6 estimations. How long were you in Vienna?
7 If you were in Vienna 3 weeks, no
8 credibility. Do you speak German? No. No
9 credibility.

10 You have to go there, you have to
11 learn the language. And that's a big
12 investment. So you have to learn computing,
13 you have to learn statistics, you have to
14 know mathematics, and you have master a
15 language and a culture. And that ups the
16 cost. I mean, I think that makes
17 comparative politics a little more
18 challenging.

19 Disciplinary history. John, I'm
20 not sure we had a Riker. God knows, I think
21 the world of John Sprague and Adam
22 Przeworski -- this is public, right? God

1 knows, Bob Bates is one of the greatest
2 human beings who ever lived, but, you know,
3 Riker was pretty special. Riker was an
4 incredibly special person. And my
5 experience with this, in intellectual
6 history in Cambridge, was in a dining room
7 at MIT, when Lucien Pye talked about the
8 crisis in sequences and development project
9 that was funded by SSRC, and how they spent,
10 at V. O'Key's urging, about 20 years working
11 on predicting when democracies survive and
12 when they develop.

13 And then these two guys at
14 Minnesota named Holt and Turner wrote an
15 article in the APSR in 1974 that said they
16 had no research design, no methods, no
17 theory, and that they had basically wasted
18 tens of millions of dollars accomplishing
19 nothing. Now I know I'm in trouble, but it
20 was my advisor, so I guess it's okay.

21 So we didn't really have a Riker
22 to take us by the hand and show us in the

1 same way. I think Adam Przeworski did a lot
2 of good, John Sprague, Bob Bates, and
3 others, but we really lack a central figure
4 in our intellectual history, and I think
5 that's an important factor.

6 The absence and inability to
7 collect data, something that Dina has worked
8 on for years, is clearly a barrier. And
9 people doing survey research in Russia, I
10 think, can talk a lot about that -- a
11 country that Frank and I tried to study
12 compare.

13 And Jim's absolutely right, what I
14 would call a resistance to reductionism.
15 Jim calls it an emphasis on macro-politics.
16 People want to write the unified field
17 theory of politics. And they want to do
18 what Bob Holt calls social astronomy -- the
19 big bang, the origin of the universe, why
20 are there revolutions, the origins of the
21 state.

22 And they're oriented that way and

1 they're taught that way and their seminars
2 are constantly posing big, giant questions
3 like that. And it never bothered Bob Holt.
4 He said, well, we're just social
5 astronomers, you know, we're doing big
6 things, we can't experiment. We just watch
7 the universe. We watch these big, big
8 phenomena.

9 But that creates a problem
10 because, of course, you can't -- you know,
11 problems of scale. Chris taught me about
12 that a long time ago. You get 13, 14
13 equations and things unravel real fast.
14 Standard errors get big real fast. There
15 aren't closed form solutions and there
16 aren't -- well, maybe there are if
17 everything's quadratic, but I don't think I
18 have to get into that.

19 The outright spots, in addition to
20 what Jim mentioned, I think he put his
21 finger on one genre that really has made
22 advance -- the study of cabinet duration, of

1 cabinet equilibria, legislative process -- I
2 stand corrected, I should have said
3 legislative process. I think John Londregan
4 is doing some of the very best work in the
5 country, and every nickel you give him I
6 think is a great investment -- as well as
7 some others.

8 I would just add that the field of
9 comparative political economy -- and I am
10 nervous, and I hope Carl corrects -- I'm
11 glad he mentioned epidemiology, because I
12 think we do spend too much time trying to
13 be -- at least I do, being like economists.
14 But there are fellows like Torben Iversen,
15 one of Jim's colleagues, who's doing some
16 very important work on decentralized
17 monetarism and what's the best form of
18 democratic governance.

19 How do you match wage bargaining
20 institutions with different kinds of central
21 bank institutions to produce room to
22 maneuver in and -- certain mixes of

1 macroeconomic outcomes that are preferable?
2 And Torben's taken a step in this direction,
3 too, although I don't think he's attained it
4 a dynamic stochastic general equilibrium
5 model. Few of us have done.

6 The barriers to advancing
7 comparative politics, data. We really have
8 made progress on data, but there's still
9 some really big challenges there, especially
10 in Central Europe, Southeast Asia, other
11 places. We just don't have polling data,
12 public opinion data, things we need to
13 advance the field.

14 There's a lack of variety. If
15 you're going to do macro studies, there is a
16 lack of variety. I mean, how many Nebraskas
17 do you have, Jim? You know, you need a few
18 more unicameral states. We need a few more
19 majoritarian countries. How many democratic
20 socialist experiments do we really have --
21 Yugoslavia? A real success, right? You
22 know, there is a problem of a lack of

1 variety. I don't know how the astronomers deal
2 with this -- more quasars or more whatever.
3 But we do have that problem, especially if
4 we're going to be macro oriented.

5 I don't know, Dick, if you can do
6 cross-cultural experimentation in the same
7 way you do it in the states. Will Arabs and
8 Israelis go into a randomly assigned
9 experiment and do all the things you want
10 them to do? I don't know if cross-
11 culturally it's quite as easy, but I hope
12 you can do it.

13 Last year, at one of Dina's
14 conferences, we saw a fascinating simulation
15 study by, I think, one of Carl's students on
16 genocide, on Rwanda. It was one of the most
17 powerful presentations I've ever seen and
18 one of the most difficult papers I ever had
19 to critique. Maybe simulations will work,
20 but when you talk about genocide in the
21 great lakes region of Africa, I don't know.
22 I mean, I just don't know if that's how you

1 attack it, with a mathematical model and
2 computational methods and so on. Yet it's a
3 vitally important topic and something we
4 have to study.

5 And I guess I'll just end by
6 saying again, I recognize these are not just
7 problems of comparative politics. The lack
8 of variety, the inability to experiment,
9 lack of data -- these are problems that
10 plague all the different fields. But I do
11 think comparative faces challenges, because
12 we have to fight this big metasplit before
13 we even get to this other question. And we
14 lack a real -- a Riker in our history, I
15 think, that really sets us apart.

16 MR. SCIOLI: Do others want to
17 weigh in?

18 MR. ALDRICH: Can I ask a
19 question? It sounds like -- I mean,
20 listening to you guys, it sounds like where
21 progress is going to be most likely made is,
22 you know, in probably advanced industrial

1 democracies.

2 All these problems seem like
3 easiest to solve there. And is it -- does
4 it make more sense to sort of emanate out --
5 if you were, oh, let's say at an NSF
6 conference trying to decide how to divide up
7 money, is that what you'd do with the money
8 rather than -- and just say, you know, as
9 incredible as it is, sorry, Africa, it's
10 just almost impossible to imagine many good
11 projects; sorry, you know, South Asia, or
12 something?

13 MR. ALT: You might want to do
14 that. That's a really good and big
15 question. The reason you might want to do
16 it, not be embarrassed to do it, is that
17 most of the time when I visit people in
18 places like Mexico, they ask me questions
19 like how should we restructure our
20 institutions so our economy will work more
21 like yours?

22 Of course, the answer to that for

1 me is I don't have a clue. That's a little
2 beyond the present boundaries of what the
3 field feels it knows. But when I think
4 about how you'd go about answering that, you
5 probably would concentrate your resources on
6 understanding the institutions in the
7 advanced industrial societies, because you
8 can't really tell people, you know, with any
9 confidence at all -- or with a complete lack
10 of confidence -- how to create institutions
11 that have certain effects unless you
12 understand the effects of those institutions
13 in their target society.

14 So, yes, you probably would,
15 though I don't think you'd put all your
16 resources there because you'd also want to
17 understand why the institutions they
18 currently had were doing what they were
19 doing. But I think that's not an outrageous
20 way to think about, you know, how to
21 actually get people elsewhere interested.
22 You can't study the transition until you

1 understand what the transition is a
2 transition to.

3 MR. YOUNG: Well, it's a huge -- I
4 mean, of course this is how political
5 scientists give up jobs to economists.
6 Because an economist would have immediately
7 said, "Well, of course we know how to do
8 that. Here's a five-point program." It's
9 completely wrong, as it turns out.

10 MR. ALT: Well, that's how we get
11 those jobs back.

12 MR. BRADY: I'd like to talk for a
13 moment about the two strategies by which
14 formal modeling can be brought to
15 comparative. One strategy we've talked
16 about. I think that's taking the
17 micromodels by and large developed in
18 American politics and bringing them to other
19 countries, like studies of legislatures and
20 so forth, or even mass behavior.

21 But there's another way to go,
22 which is -- I might characterize as the Mike

1 Wallerstein kind of approach. And Jim
2 Robinson, my colleague at Berkeley who we
3 recently hired takes this approach. The
4 models look like growth theory models or
5 macro models. They usually have very simple
6 notions -- there's two classes in society,
7 or maybe three if you're getting really
8 complicated. And then those models have the
9 great virtue that comparativists actually
10 think that way, so that Jim can talk in our
11 department to a lot of comparativists who
12 would find the micro modelers impossible to
13 understand, and who also would be dealing
14 with problems that a lot of the
15 comparativists don't find important, which I
16 think is a mistake on their part. But
17 nevertheless, that's the way they feel.

18 But Jim's made a tremendous
19 bridge, I think, to the comparative faculty
20 in our department. I have some skepticism
21 sometimes about these macro models for all
22 the reasons people worry about macro models,

1 but it sure is one strategy, I think, to
2 engage comparativists. And I would hope
3 that we at least push that somewhat farther
4 and see where it's going and make sure that
5 we don't give it up.

6 That has also gotten some impetus
7 from the fact that economists now are more
8 and more concerned with some of these issues
9 with the collapse of the Soviet Union and
10 Eastern Europe, and they're trying to
11 discover explanations for what went on --
12 some of which are micro, by the way, but
13 many of which are also macro kinds of
14 questions. So there's real points of
15 contact there between economists and
16 political scientists as well.

17 MR. SCIOLI: Other observations?
18 I was struck at the discussion between the
19 big problems and the attack on the big
20 questions and political science's reluctance
21 and certainly Becky and Dick have
22 experienced this over the years. And I

1 guess we had criticized the psychologists
2 for the kind of small questions, certainly
3 the short prefaces to their articles, 12
4 pages with here's the problem, you know all
5 the previous research that's been done on
6 it, here's the method, here's the
7 experiment, and here's the outcome. I mean,
8 can we have it both ways?

9 MS. ZINNES: No, but you can
10 publish a lot more articles that way.

11 MR. SCIOLI: At a cost. Is this
12 an appropriate place for a break? Norman or
13 Bill, any questions before we leave this
14 section?

15 MR. BRADY: Could I ask where
16 Bill's going off to? You said he was
17 leaving --

18 MR. SCIOLI: Sure, if you want to
19 gossip, that's great. Let's get a cup of
20 coffee and Bill can --

21 MR. BUTZ: I'm going to a small
22 private consulting firm that does work in

1 less-developed countries -- Eastern Europe,
2 former Soviet states. And they -- AID,
3 Asian Development Bank, and all that, they
4 specialize in restructuring central banking
5 systems, statistical systems, and micro
6 finance, and I'm going to be responsible for
7 trying to build up a capability of poverty
8 monitoring analysis and policy, and also
9 responsibility for the statistical system.

10 MR. BRADY: Good luck.

11 MR. SCIOLI: There is an NSF panel
12 -- a mission-like panel, I guess, will be
13 constituted by our human resources folks.
14 The competition for that position has
15 closed. And I assume in several weeks,
16 Norman, we'll begin to get the applications
17 and begin to think hard about who might --
18 none of that has happened yet, is that
19 correct, Norman?

20 MR. BRADBURN: Oh, no -- yes, it
21 has -- I mean, we have -- all the
22 applications are in. They're being -- a

1 first evaluation's been done by the upstairs
2 people, which sort of weeds out the —
3 incompetent. They should begin the serious
4 work of winnowing it down and bringing the
5 people in to talk, I hope by the end of the
6 month.

7 MR. SCIOLI: Well, we have coffee.
8 We'll resume in 15 minutes.

9 (Recess)

10 MR. SCIOLI: Gentlemen, can we
11 resume?

12 MR. MORTON: And ladies.

13 MR. SCIOLI: But you're seated.

14 MS. MORTON: Well, what about
15 Dina?

16 MR. SCIOLI: An important piece of
17 housekeeping. I'm going to circulate an
18 attendance sheet. Please -- this is the
19 means by which you become remunerated. And
20 if you don't mind, just to be sure, print
21 your Social Security number. The government
22 payroll office will not release a check

1 unless we have a correct --

2 MR. SIMON: But if we see each
3 other's Social Security number, there's --

4 MR. SCIOLI: You can provide it to
5 us privately, if you would.

6 Also, it's our objective at NSF to
7 extract as much from you as we can while
8 you're here. And I was talking to Cheryl
9 during the break and then I approached Bill
10 Keech directly.

11 Jim and I can think of 20 more
12 people that we might invite to workshops.
13 But it's the next 20 after that that we're
14 going to have trouble with. And it's the
15 younger scholars. I mean, now that we've
16 identified this as being the over-the-hill
17 gang -- and that's keeping it in layers.

18 (Laughter)

19 Could you, either now or at a
20 later point, e-mail us or scribble down on
21 the pads names of others whom we might
22 invite in subsequent months to participate

1 in workshops, particularly junior folks who
2 maybe are getting the kind of training we're
3 talking about here and who could be
4 excellent resource persons, et cetera.

5 MR. KEECH: Tenured -- or junior
6 tenured?

7 MR. SCIOLI: Either way. We'll do
8 our background checks as appropriate.

9 MR. SIMON: How did this group
10 pass?

11 MR. SCIOLI: Well, you know --

12 SPEAKER: Don't answer that. Take
13 the Fifth on that.

14 MR. GRANATO: We'll start with
15 international relations -- Dina?

16 MS. ZINNES: Okay. First let me
17 just say what a pleasure it is to be amongst
18 this group of people. I have to admit that
19 when I got those initial memos I sort of put
20 them on the side burners, thinking, well,
21 okay, I'll look at them eventually, because
22 I was worried about the fights and the

1 antagonisms that I thought would emerge.
2 And it was with great delight that I read
3 those and discovered, my gosh, there really
4 is a consensus going on here.

5 And listening to people this
6 morning confirms that. I find that it's
7 wonderful to see that both the empirical and
8 statistical side and the modeling side
9 really all sort of agree on certain things.
10 And I think that's a fabulous beginning.

11 So it's a real pleasure to be
12 here. Why is there a split in IR? Well, I
13 think I addressed that in the memo and it's
14 been said a number of times here. It's
15 historical. It happened that way. I think
16 what happened was people came into the
17 profession with a mindset that was much more
18 quantitative, empirical-based, and that just
19 spread -- it was easier to spread.

20 But I think it is perpetuated now
21 by confusion -- a confusion over what is
22 mathematics, what is statistics versus

1 mathematical modeling. They're all math,
2 right? Statistics is a branch of
3 mathematics, after all. What is theory?
4 What is a model relative to theory? And how
5 do you do science? What is science?

6 I mean, people thought for a long
7 time that science was counting things,
8 collecting data, looking at empirical
9 observations.

10 And in IR that really was an
11 important step, because we did have
12 "theories," big theories -- balance of
13 power, you know, realism, idealism, all
14 those wonderful big things which were very
15 ill-defined. But nobody looked at
16 observations.

17 Nobody went out and actually
18 examined the way nations interacted with
19 each other. And the notion that you could
20 do that was a really important notion. And
21 so in came the huge quantitative influx.

22 And I think once it was there, I

1 think people were really very confused over
2 what's the difference between doing --
3 testing a hypothesis and actually setting up
4 a theory. For a long time those were one
5 and the same thing. A theory was simply a
6 hypothesis, right? And then you go out and
7 you sort of gather data and set up a null
8 and test it and so on.

9 So I think there has been a
10 tremendous amount of confusion. And I think
11 the confusion continues. I'm struck by Jim
12 Alt's comment that it's really very, very
13 hard to do political formal modeling.
14 Frankly, I think it's very, very hard to the
15 statistical thing nowadays, too. I long
16 since gave up on some of the fancy
17 terminology and technical aspects of that
18 area.

19 I think the problem that I see
20 right now is -- and this is why I think
21 people feel these things are hard -- is that
22 we have lost sight of the most basic

1 ingredient here, which is the ideas, the
2 theory. That is to say, we no longer -- we
3 think in terms of what I call cookie
4 cutters. And I think this is true in the
5 modeling world as well as in the statistical
6 world -- you learn, what, I don't know what
7 is the latest thing in time series analysis.
8 Or you learn something in theory, the
9 limited information or whatever. And you
10 squeeze your problem into one of those
11 things, and it doesn't matter whether it's a
12 time series equation or a game theoretic
13 model.

14 We think in terms of theory as
15 having these little cookie cutters that we
16 take and we push our problem, our question
17 into those cookie cutters. And whether it's
18 a model or a statistical model doesn't
19 matter, because once you start thinking that
20 way, yes, it does get very, very hard.
21 Because then you really do have to sort of
22 move up there in terms of the advancements

1 of game theoretic models or in terms of
2 statistical models.

3 And I think we have to go back to
4 realizing that what is fascinating and fun
5 for us is the theoretical aspect. That is,
6 thinking in terms of -- and I'll use Jim's
7 terminology -- thinking in terms of puzzles.
8 I don't think we train students to think in
9 terms of puzzles.

10 I think we tell them a lot about
11 literature and I think we tell them a lot
12 about statistics and we say go find thee a
13 problem and stick the two together and make
14 up a dissertation.

15 And I don't think that's the way
16 you advance science. I think you have to
17 think in terms of things that are puzzling.
18 Why are you interested in something? And
19 that -- you know, the student goes through
20 political science training, comes up against
21 a dissertation, and says now what? Why does
22 he come to that point?

1 I would say he hasn't got any
2 questions. He has an interest -- he may be
3 interested in Congress or he may be
4 interested in war or conflict or something.
5 But he hasn't got a question. And I think
6 the reason he hasn't got a question is
7 because we don't train him to think in terms
8 of questions.

9 I think we have to start -- and I
10 won't speak to other fields, but I think in
11 international politics we have to teach
12 literature -- which I think, you know, the
13 student has to know that -- from the
14 perspective of seeing things that are
15 puzzling. Well, what's puzzling? Puzzling
16 is something that doesn't fit with the way
17 everybody has been thinking about the
18 problem. Why is it democracies don't fight?
19 That issue is so interesting. Well, because
20 democracies do fight, but they don't fight
21 other democracies. Now, that's sort of
22 puzzling, and that begins to pose a

1 question.

2 Why is that important? Because
3 it's out of those puzzles that comes theory.
4 What is a theory? To me, a theory is
5 nothing more than a story. It's a story
6 that you tell yourself in an effort to try
7 to put into context something that doesn't
8 go together. And if we would go back to the
9 notion of those stories -- and when I train
10 graduate students, I try to point out to
11 them that you are doing this all the time.
12 Every time you practically walk down the
13 street, read a book, see a movie, whatever,
14 you are constantly faced with things that
15 don't quite make sense from your
16 perspective. And that triggers you to start
17 thinking about what is going on. That
18 trigger then leads you to a story. And that
19 story, that explanation, is the theory.

20 Now, what's a model? A model --
21 to me, mathematics is just a nice way of
22 being able to translate that idea, that

1 story into some form that allows you to
2 generate some conclusions. Why do it that
3 way instead of verbally? Because there are
4 rules. People spent years and years and
5 years, centuries, developing rules that will
6 tell you how to get from here to there. If
7 you set it up in this form, you come out
8 with these kinds of conclusions.

9 But I think the problem is we
10 teach people the statistics and even the
11 models from the perspective of a method
12 instead of from the perspective: Here's a
13 question you're interested in. Okay, here
14 is a puzzle, here's a story you're trying to
15 tell. Now, you've got the story elaborated.
16 This is your explanation.

17 How do you take that and translate
18 it into something that will get you
19 somewhere? And I think it's a terrible
20 shame that we think to a large extent -- in
21 IR, certainly, and probably in general -- in
22 game theoretic terms. There's more out

1 there in terms of the modeling languages
2 that we have available. I think there are
3 lots of interesting ways to cast problems.
4 Yes, if you think in terms of voters,
5 congressmen, there are strategic factors
6 involved, maybe game theory is the right way
7 to go. But I don't think we should shut it
8 off at that point.

9 But we don't have any way of
10 training people to be even aware of the
11 extent to which there are these other
12 languages that we could use, other models
13 that we could put our questions into so that
14 we could then derive something more
15 interesting.

16 So I -- my feeling is that we
17 teach people these methods, whether they be
18 game theory or whether they be statistical
19 models, and then they search around for some
20 kind of a question and shove it into that
21 format.

22 I would like, personally, to see a

1 training program at the graduate level in
2 which you start the student by thinking of
3 questions from a puzzle standpoint. I've
4 done that in a number of graduate classes,
5 where I talk about something -- you know,
6 artists have little sketchbooks, they go
7 around, they sort of see somebody and then
8 make a little sketch of that person, and
9 they use it later on in another situation.
10 And I have them keep a "sketchbook" of these
11 sorts of puzzles. And I say, look, when
12 you're reading your literature in your
13 literature classes, ask yourself does it
14 make sense to you. And if it doesn't, why?
15 Could you explain it?

16 The interesting thing is a lot of
17 these people have an awful time finding
18 these puzzles because so much of the
19 teaching of literature is from the
20 perspective of who said what, okay, and how
21 that builds on who said what before, et
22 cetera, as opposed to what's puzzling about

1 this whole thing.

2 So I would like to start the whole
3 training program from the perspective of
4 teaching people, teaching graduate students
5 how to pull questions, puzzles out of the
6 literature. And then, once they begin to
7 get that -- and I would just do it verbally.
8 That is, don't get them so tangled up in
9 fancy mathematics. Just start verbalizing
10 what it is you want to say.

11 Once you've got that, then look at
12 the way the story operates; that is, what
13 are the important ingredients of the story.
14 And search for an appropriate forum, an
15 appropriate language, mathematical language,
16 that will capture the key ingredients of
17 your story.

18 I think it would be fascinating to
19 take political science questions and see if
20 we couldn't sort of cast them in generic
21 terms as having certain kinds of fundamental
22 qualities. And then look across into the

1 mathematics, theories of mathematics, and
2 see what kinds of qualities of those
3 languages parallel the ones that are in the
4 political science kinds of questions.

5 In mathematics, you know, I mean,
6 we do a lot of game theory, strategic
7 decision making, rational choice sorts of
8 things. We don't do very much with dynamic
9 systems. There's practically nothing that's
10 been done with graph theoretic notions,
11 which I think have fabulously interesting
12 potential for some of the kinds of questions
13 we want to ask.

14 So I would then take the student
15 and try to show them the variety of
16 mathematical languages that exist with
17 respect to the kinds of problems that they
18 could match them with.

19 And then finally, I mean, you do
20 have to train the students to do something
21 mathematically. So there's no question that
22 you either have to -- you don't "either" --

1 you need some mathematical background in
2 order to know how to take it the next step.
3 And I think we need there to really think
4 long and hard how we can short-circuit the
5 becoming a PhD in mathematics problem. If
6 you can't do that, that's all there is to
7 it. But we can do other things that are
8 short of that.

9 And then, I think, the statistical
10 component here -- I think of statistics not
11 as a mathematical model, but maybe that's my
12 limitation. I think of statistics from the
13 perspective of a mechanism for making
14 decisions about data after I have my theory.
15 And the theory then tells me what to look
16 for in terms of the deductions, and then I
17 go out and collect data. And the
18 statistical part is a way in which it tells
19 me how much confidence I can gain about my
20 theory based on this data that I have.

21 And I don't think -- and somebody
22 said it earlier here -- sometimes you don't

1 need the fancy statistical -- sometimes you
2 might, I mean, I'm not going to rule that
3 out. But sometimes a cross-tab (?) is
4 perfectly legitimate, or just seeing whether
5 or not the sun rays go a certain direction.
6 I think that the statistics should not
7 dictate the model. I think the model -- I
8 think you start with the puzzle, you then
9 develop the story, you then translate it
10 into a form that allows you to do something
11 with it according to certain rules that
12 people have already set up and found to
13 work, and then you start collecting data and
14 you ask yourself, now, how do I analyze
15 these data in a way in which I can then gain
16 some confidence in what I've said.

17 And the reason I think the split
18 needs to be pulled together, the reason I
19 think that's so important, whether it's
20 natural or not, is, from my perspective you
21 don't want to just be a theorist.

22 You don't just want to be a -- at

1 least I don't want to be just a math
2 modeler. I want to talk about the world. I
3 want to talk about nations and how wars
4 occur and how conflicts occur -- how one
5 goes into the other. I want to know whether
6 or not my argument, my story makes any
7 sense. And so I want to go out to the real
8 world and I want to know how to collect data
9 and I want to know how to actually process
10 it so I get reasonable answers.

11 So I think we need to totally
12 change the whole graduate training program.
13 Now, I'm maybe optimistic, but I'm not an
14 idealist. I don't think it's going to
15 happen. And I think this is where the
16 National Science Foundation has a marvelous
17 opportunity, because I think departments are
18 so, sort of, frozen into certain ways of
19 doing things.

20 We're changing, but it will take a
21 long time. And it will only be by seeing it
22 happen somewhere else. And I think you see

1 it happen through things like various people
2 have mentioned, workshops, summer workshops.
3 I think those sorts of things are the way to
4 do it.

5 You do it bit by bit. You have
6 perhaps a rotating summer workshop
7 structured in certain ways so people begin
8 to get the idea that this is a good thing to
9 do, and so on.

10 I think you also need some
11 conferences in which we talk specifically
12 about what does it actually mean to test a
13 model, a mathematical deduction? I don't
14 think those things are at all obvious.
15 Bueno de Mesquite wrote, is it War and
16 Reason in which he makes a valiant effort to
17 develop a very interesting game theory
18 model, and then allegedly pulls from it
19 certain deductions which he empirically
20 tests.

21 The idea is excellent. The
22 trouble is oftentimes those deductions are

1 really -- it's not clear how those
2 deductions follow from the model. Once in a
3 while they do, but a lot of times they're
4 kind of auxiliary to the model.

5 Which actually brings me to
6 another point, which is that I don't think
7 we often understand really what a deduction
8 is from a model. That is, you know, what is
9 it that you test? And that brings me to yet
10 another aspect here, and this is where we
11 desperately need the help of statisticians.
12 When you test something, the idea is to
13 be -- you want it to be set up so that you
14 can reject it. Okay, that's the goal. And
15 if you don't pass that goal, you're very
16 happy but that's what you're trying to do.
17 You're trying to get confidence in your
18 argument. And you say, well, let me stack
19 all the cards against me and if I can still
20 make it, then I'll really have some
21 confidence in this story.

22 The problem is it's difficult to

1 know how you do that. For example -- let me
2 just give you a quick idea -- the Richardson
3 Arms Race, which I'm sure most of you are
4 familiar with -- two linear differential --
5 a couple of linear differential equations,
6 and people have played with those for years
7 in the discipline. And the question is how
8 do you test it.

9 Well, you know, the usual
10 regression notion is you just go out and do
11 each regression equation on each of the two
12 equations. But Richardson tested it by
13 "drawing the deduction," that you add the
14 two together and you get a linear something
15 or other and then you go out and look at the
16 linear aspect.

17 Well, okay, that's kind of a minor
18 notion of a deduction. There are more
19 interesting kinds of notions of deductions
20 where, if the parameters of the model are of
21 certain magnitudes, the system is stable,
22 which means that you're moving towards an

1 equilibrium point.

2 Now, suppose you go out and take a
3 set of data, you actually estimate the
4 coefficients of those equations, and now you
5 look to see whether or not that arms race is
6 actually stable -- that is, is it moving
7 towards an equilibrium point, which you can
8 define having given the values of those
9 parameters?

10 But to what extent, if you've
11 actually extracted the parameter estimates
12 from the set of data, are you biasing your
13 test when you go out to look to see whether
14 or not it's moving towards the equilibrium
15 point? I don't think that's at all obvious,
16 and I've played with that for years. So
17 there are things like that, that you want to
18 set the test up so that you are really -- so
19 that it's not circular.

20 So I think we need discussions of
21 how you do that. I think we need
22 discussions between statisticians and

1 mathematicians. But I'd like to see those
2 structured from the perspective of actual
3 problems -- not generically, how would you
4 test a game theory, no; but rather, here is
5 a model -- okay, well, here is a particular
6 project and here is a model. And we
7 probably could get data this way. What
8 would you suggest as a way of actually
9 testing that? So I think that would be yet
10 another way to go about this.

11 And then finally, let me just
12 conclude a little bit with a point that
13 Becky brings up and was reiterated several
14 times now, namely the need to get into the
15 undergraduate scheme of things. I've come
16 to that conclusion, too, after years and
17 years. But I think -- well, let me tell you
18 what I've done in the last, say, half-dozen
19 years.

20 I teach a freshman course, it's
21 called a discovery course, so it's a limited
22 enrollment. And my goal in that course

1 started out to be how to show freshmen, who
2 are coming in -- it's a required course, you
3 get, you know, required credits for it. How
4 do you show them that political science can
5 be analytic? How do you show them that that
6 is a possibility and moreover that it's
7 important? And even if they're not going to
8 be political scientists, but just as
9 ordinary citizens they have to make
10 decisions about things, and that those
11 decisions are actually based upon a bunch of
12 assumptions. And what are those
13 assumptions? They're assumptions about the
14 way the world works. They're your theory
15 about the way the world works. If you're
16 wrong, you're going to be in trouble.

17 So I get them through a program of
18 teaching them how to actually -- it's
19 essentially a model, but I never use those
20 words; using propositional calculus, just
21 simple logic. And we've developed a,
22 actually a piece of software that will allow

1 us to do this without their learning a whole
2 lot of actual logic manipulation. And by
3 gosh, you can show them how the assumptions
4 you make do or don't lead to certain
5 conclusions. And then, actually, how you
6 might go out and test some of those -- what
7 kind of data would you actually collect in
8 order to check those out.

9 Well, I've tried to evolve this
10 course more and more now in terms of public
11 policy, because I don't think many of these
12 kids are ever going to become political
13 scientists. But they will become decision
14 makers and citizens, and they do have to
15 make decisions. And if they can see the
16 importance of that, that will be very
17 significant.

18 But what's happened with this
19 course over the years that I've been
20 teaching it is that, first of all, there's a
21 huge standing in line to get into the class.
22 But more interestingly is the fact that the

1 students that are coming into the course are
2 largely now coming from the sciences,
3 mathematics, engineering, and so on.

4 And why is that? Well, for one
5 thing, you know, they're told by advisors
6 who apparently have heard about this that
7 this is a course that is analytical, which
8 is sort of along their training lines; and
9 secondly, satisfies a requirement; and
10 thirdly, uses computers. It's all
11 Web-based. So the students come in and
12 they're absolutely fascinated by the fact,
13 my gosh, you can do these analytical things
14 in political science? Yes, you can.

15 And I think that's the beginning
16 of attracting these kids. Because what is
17 happening is -- I mean, yes, we need to
18 train people who are going into social
19 science so that they have more analytical
20 background. But I think we also need to
21 entice those who have that inclination
22 already and some very good background to

1 come into the field. In the past we haven't
2 done that to a large extent. Who goes into
3 political science? People who didn't do
4 very well in other places.

5 So those are my thoughts about why
6 we're here and what we might be able to do.
7 And I think, as I said, the National Science
8 Foundation has a really potentially unique
9 role in this, because I don't think it's
10 going to be done by universities by
11 themselves. They need a push like crazy.

12 MR. GRANATO: Would anybody like
13 to add to that?

14 MS. MORTON: I have a question.
15 It seems to me that, in my experience at
16 Iowa and some at Houston, that what happens
17 is the people who do modeling and methods
18 end up teaching those courses because there
19 are very few people that do that, and then
20 the other courses that are more substantive
21 end up being taught by people that don't
22 really do it. So when they do do articles

1 that are more methods or modely, they don't
2 go through them. They just kind of say read
3 the intro, read the conclusion.

4 And so the students are getting --
5 they're not -- when they take the
6 substantive classes, it's all critique of
7 the literature. And then we get the
8 comprehensive exams, and the comprehensive
9 exams reinforce that.

10 So they've spent all this time
11 studying for that. And that's sort of why,
12 when it comes to dissertation stage, they
13 don't, you know -- aren't ready to think
14 about how -- they may have ideas, but they
15 don't think about methods and particularly
16 models, how they might approach it from a
17 modeling standpoint. And I was wondering if
18 you had that feeling.

19 One thing, at NYU apparently they
20 don't give comprehensive exams, and I
21 haven't quite figured it out whether I think
22 this is a good or bad thing. So I was just

1 curious as to what you think about that
2 part.

3 MS. ZINNES: Yes, I think all of
4 those things feed into this; the training we
5 get or the training we provide for the
6 graduate students is so segmented.

7 You learn the literature, but you
8 learn the literature from the perspective of
9 who said what. You learn the methods, so
10 you know how to do regressions and whatever.
11 You don't put those things together,
12 typically. Now there are some attempts in
13 some of these -- what do they call them?
14 Research design-type seminars.

15 But it's very mechanical in the
16 sense of, you know, you come into this
17 course and you're supposed to just
18 essentially collect data and do a
19 statistical analysis. It's independent of
20 any kind of theory, although it doesn't have
21 to be. But typically, the student comes in
22 and all he's got is kind of, well,

1 this-goes-with-this kind of a notion, from
2 the reading. So then they talk about how do
3 you collect data and they do the research
4 and content analysis and so on. So yes, I
5 think all these things are part of the way
6 we train students. The reason they come up
7 with the big question mark at the end is for
8 that reason. Everything's a hypothesis.

9 But one of the things that I think
10 is so important here, we need to get some
11 of -- to go back to Jim's point -- we need
12 to get some of the fear out of all of this.
13 Theory building is not necessarily knowing
14 huge amounts of mathematics. It may need
15 some, yes, I agree. But you don't really
16 have to be a dyed-in-the-wool mathematician
17 to be able to do theory. You're telling
18 stories. You need to translate those.

19 Propositional calculus has been
20 for me a very, very interesting way to go,
21 because it is so simple. It is so
22 incredibly simple. Now, its simplicity also

1 means it has a lot of problems. But that's
2 also nice to show people because that shows
3 the extent to which some modeling formats
4 are more appropriate for certain problems,
5 certain questions, and others are better
6 suited.

7 But I think the students need to
8 realize that they don't have to be so
9 terribly sophisticated initially. Now,
10 obviously we'd like to get them trained
11 better and better, and hopefully over the
12 years we'll do that. But initially we need
13 to start getting them thinking about how you
14 formulate the theories.

15 MR. GRANATO: Anybody like to add
16 anything else on this topic?

17 MR. SIMON: As a dyed-in-the-wool
18 mathematician, I was thinking about
19 something that you said and I think maybe
20 Becky said also about one of the --
21 something I was going to expand on a little
22 later when I compare math and political

1 science.

2 There really is a difference in
3 the quantitative background of the students
4 who go into economics versus students that
5 go into political science. I see that when
6 I chair public policy graduate students.
7 And there are students that say whether they
8 want sort of the econ track or the political
9 science track. It's night and day, right?
10 The math folks all go to the political
11 science track. Those who've had calculus or
12 something go the econ track. And I suspect
13 the same thing happens at the PhD level.

14 I teach the course in the
15 economics department at Michigan, or often
16 do, on math techniques. In order to get
17 into the PhD program, you have to have at
18 least three math classes, undergraduate
19 course, background. My impression is many
20 political science applicants have no math
21 background and they're choosing political
22 science because of that. I think that's

1 maybe really at the heart of some of the
2 issues we're dealing with.

3 MS. ZINNES: But it's a little
4 deeper than that. It's not just math
5 courses. Math courses -- they don't have
6 math courses by choice usually. Why is that
7 and what does it mean in terms of the way
8 they think? It means that they are not
9 analytically inclined. I mean, the math
10 course -- knowing calculus is great, but
11 it's the reasoning process that goes on in
12 learning those, how you prove things and so
13 on. That's the part that we're missing.

14 MR. SIMON: Right. You said
15 something -- right. I actually meant to say
16 it that way. I think many of the students
17 that I see moving into political science are
18 math phobic. It's not their background.
19 And some have had calculus and, you know,
20 don't even know what they did there. But
21 it's the math phobic types that -- you know,
22 there's a place for them in economics and

1 political science, but it's -- I'm just sort
2 of struck by the huge difference in those
3 two areas.

4 MR. MCKELVEY: But is that really
5 true? Say the schools that are represented
6 in here, I mean, I know we require a math
7 background, you know, a fairly substantial
8 math background in the students we admit.
9 But, I mean, is it true that in the
10 political science programs you don't require
11 that?

12 MS. ZINNES: No.

13 MS. MORTON: Most political
14 science programs don't require any math
15 classes as part -- and there are many
16 political science programs that don't
17 require any methods. You can get out with a
18 quantitative methods class. If you do
19 political theory you don't have to do it in
20 a lot of programs.

21 MS. ZINNES: Are you talking
22 undergraduate or graduate? Because I'm --

1 MS. MORTON: I'm talking about
2 graduate -- and then much less have to have
3 it to get admitted. I mean, if you don't
4 have to take it while you're there, why do
5 you need it before you -- but, you know, at
6 Iowa we had a required math class and we
7 were an exception. I mean, there's some
8 required math class. Anybody else have the
9 required math class in their political
10 science? There's no required math class at
11 NYU, there's none at Houston.

12 MR. McKELVEY: You mean a required
13 math class once you get there --

14 MS. MORTON: Yes.

15 MR. McKELVEY: Or to be admitted?

16 MS. MORTON: Much less before you
17 get there, but --

18 MR. SIMON: And the economics
19 course that I teach that's required, very,
20 very rarely is a political science student
21 in it. And they're almost all Chris Achen's
22 students. They're all Chris Achen's

1 students as far as I know.

2 MR. FREEMAN: Dina, how can you
3 explain the fact that if we assess our
4 average GRE scores in
5 mathematics/analytical, I'll bet they're all
6 750, 780.

7 MS. ZINNES: It just shows they
8 have the capability.

9 MR. FREEMAN: But then they have
10 the analytic capability -- those tests --

11 MS. ZINNES: They have -- no, I
12 think they have the analytic capability.
13 It's never been developed.

14 MR. ACHEN: Econ and poly sci have
15 been about equal on math GREs for 30 years.

16 MS. ZINNES: Is that right?

17 MR. ACHEN: It's shocking to me
18 that econ and math -- sorry, econ and poly
19 sci, I said the wrong thing -- econ and poly
20 sci math GRE scores on average are about the
21 same.

22 MR. SCIOLI: You mean at Michigan

1 or overall?

2 MR. ACHEN: Anywhere. So it's not
3 raw talent.

4 MR. BRADBURN: Well, I think it's
5 the case that -- and I can't remember
6 whether this is all graduate departments or
7 some selection, but I think it's all -- that
8 if you combine engineering and math, the
9 modal undergraduate degree now for people
10 entering a graduate program in economics is
11 that combination.

12 There are more undergraduate
13 majors in mathematics -- Greg, do you happen
14 to know -- in mathematics and economics than
15 there are -- excuse me, in mathematics and
16 engineering than there are in economics, who
17 are going into graduate programs.

18 MR. YOUNG: Well, of course, this
19 is -- something like that is right and it's
20 alarming to the economics profession. I
21 mean, it's absurd to say that you're not
22 able to train undergraduates to go on into

1 your own graduate courses, but that's
2 exactly what's happening.

3 MS. MORTON: Your training is much
4 more analytical than -- I mean, we don't
5 come close to that.

6 MR. ALDRICH: So the inference,
7 then, is -- I think what Chris is saying
8 particularly is that the only difference is
9 advanced training going in.

10 MS. MORTON: Yes, and I think --

11 MR. KEECH: Motivation.

12 MR. SIMON: Motivation, yes.

13 MS. ZINNES: And, you see, I think
14 that's a critical ingredient here. I think
15 as long as you compartmentalize these
16 things -- so you take your math courses or
17 your methods courses and then you take your
18 substantive courses, you don't see why it's
19 important to be an analytical thinker, a
20 theoretical thinker. And yet mathematics
21 helps you in that regard.

22 The value is it's not just to be a

1 mathematician. It's not just to be able to,
2 you know, publish articles on Markov chains
3 and whatever. The issue here is what can it
4 do for you? And I don't think we've really
5 convinced people how important it is.

6 And again I go back to the
7 propositional calculus. Because you can
8 show in such a simple fashion why being
9 analytical, just having a couple of simple
10 rules that you follow, you get somewhere
11 which you couldn't have gotten to otherwise.
12 Or you can show that somebody can't get to
13 where they think they got. And that, I
14 think, is extremely important.

15 Once the student begins to see
16 that and realize that these mathematical
17 forms are important to them, then I think
18 that -- because they have the talent. They
19 have the capabilities. They just don't have
20 the motivation.

21 MS. MORTON: I think it's
22 definitely motivation not -- I mean, for

1 instance, you know, one person who uses very
2 sophisticated mathematical methods is Greg
3 Wawro, and he told me he went to Cornell
4 because they had no math requirement. Once
5 he got there, he got plugged in to Walter
6 Mebane and changed his tune.

7 But that doesn't always happen.
8 Most of the students in -- there are
9 political science faculty who have math -- I
10 mean, I know one particular person who
11 worked as a physicist for many years and now
12 does international relations from a very
13 constructivist, anti-theory math person -- I
14 think a few people know who I'm talking
15 about. And this person -- there's no
16 question that this person has the math
17 ability, but doesn't -- isn't interested in
18 looking at -- so it's a motivational issue,
19 and I think it becomes -- it happens at
20 undergraduate levels, because -- you know,
21 when I was an undergraduate, I didn't --
22 when I got the kind of modeling of, you

1 know, social science, it was in economics
2 classes. So, you know, that's where I went.
3 I mean, I was never interested in farms and
4 prices and markets, but I persevered with
5 that because I was interested in the
6 modeling of human behavior.

7 MS. EAVEY: I think in some sense
8 you've identified two separate but perhaps
9 related problems. When you're talking about
10 raising the level of sophistication within
11 the discipline, I think that's a public good
12 that obviously will lead to more
13 sophisticated analyses and perhaps more
14 sophisticated modeling.

15 But that's not necessarily
16 capturing what we're talking about when
17 we're referring to the divide between
18 modeling and empirical analysis. Case in
19 point: I've been working with Chuck Manski
20 on setting up a MacArthur-type network on
21 the empirical implications of social
22 interactions, or something like that --

1 doesn't sound too much like Jim's acronym.
2 But Chuck claims that theory is way ahead of
3 empirical work in this area, that the two
4 groups don't talk to each other, and one of
5 the things we're trying to do is facilitate
6 that.

7 Okay, that's basically in the
8 field of economics where supposedly we have
9 a higher level of mathematical
10 sophistication. That doesn't seem to be
11 affecting this problem of the theorists not
12 talking to the empirical folks, and how the
13 different areas are developing at different
14 rates.

15 So I think you've got a couple of
16 different issues going on here that probably
17 are related, but also can be thought about
18 separately and perhaps have some different
19 solutions.

20 MR. ALDRICH: Another way in which
21 they may be related is that if you come to
22 graduate school, even if you had the

1 inclination to start, if you're starting to
2 learn analytic methods of any sort only
3 then, it may be a lot harder to get any
4 competence in anything but one specific,
5 very specific aspect. Whereas if you had
6 some in advance, you'd have the flexibility
7 to be able to learn enough to be able to
8 talk across the divide.

9 MR. ALT: One thing that I'd like
10 to share -- it picks up on what John, Chris,
11 and Becky have said -- we've had some
12 success in our graduate program in recent
13 years with a math "prefresher" we call it,
14 which is an intensive course that just runs
15 for a couple of weeks and is intended for
16 the incoming graduate students.

17 I mean, we all know that you can't
18 learn that much math for the first time in 2
19 weeks, so I think, you know, Chris has to be
20 right, so to speak. But the people we are
21 getting have already done the math and it's
22 much more a matter of just reminding them of

1 what they did. And this is, I think, the
2 problem Becky raises -- and telling them for
3 the first time why they're going to need it
4 in political science.

5 I'm not convinced, therefore, that
6 it's a motivational problem except in a very
7 wide sense of motivation. But I do think
8 that to far too great an extent people's
9 exposure to political science as
10 undergraduates does not lead them to expect
11 any of what they get in graduate school,
12 whether it's the analytic reasoning or the
13 simple instinct of taking a problem to data,
14 you know, rather than looking in last week's
15 Newsweek for, you know, something canned or
16 giving up on the state of human knowledge on
17 a topic.

18 I actually think we're potentially
19 in better shape than we think we are and
20 that attending to the undergraduate
21 foundation is more -- could really have a
22 big payoff. Because I think we are already

1 getting a lot of the right people.

2 MR. SCIOLI: How about the GREs,
3 Chris, for the difference between social
4 behavioral and the physical sciences? Are
5 they tremendously --

6 MR. ACHEN: It's been a few years
7 since I've looked at this. What I remember
8 is that it's the physicists who are number
9 one and the ed school people that are at the
10 bottom.

11 MR. SIMON: The teachers of our
12 kids.

13 MR. ACHEN: My last one just
14 graduated, so this isn't a personal crisis
15 anymore. But it's more varied and uneven
16 across natural and social science than you
17 might guess.

18 MR. SCIOLI: Let me pick on you
19 with one more question. Does the summer
20 program still -- is there any effort in the
21 ICPSR summer program to do theory?

22 MR. ACHEN: Yes. There's been --

1 it's been 15 years ago, I guess, when that
2 started and it's extended every year in more
3 and more courses. I think there are four or
4 five courses now out of whatever, 20 or 25,
5 that they offer that are formal theoretic in
6 character.

7 It's not primarily a summer
8 program dedicated to that in particular, so
9 it's always going to be something that's
10 done in addition to other things. But it's
11 a large and growing part of the curriculum,
12 and those classes are well-attended.

13 MR. SCIOLI: I mean, this is such
14 an atypical group, any question I think
15 about each of you -- I hope each of you has
16 two other colleagues with whom you are
17 conversant on these issues and with whom you
18 talk about offering courses. But again, the
19 question Jim and I were puzzling about, and
20 including Cheryl -- are there 40 more people
21 who are in departments who have two
22 colleagues with whom they have these kinds

1 of conversations?

2 And here we're talking about, you
3 know, one of the largest directorates in the
4 foundation, the science education
5 directorate, probably getting them
6 fundamentally involved in terms of the very
7 practical notion of thinking about problems
8 just to get to the graduate student level --
9 have to think about students perhaps taking
10 an 8-week or 6-week summer course at
11 Michigan or to go to the political methods
12 workshop where we're trying to encourage
13 more junior people to come.

14 It seems then, you know, the
15 terrain is treacherous and very sloggy in
16 terms of making any kind of a gain.

17 And here we're -- I guess this is
18 the choir. I'm so fearful that we're
19 preaching to the choir. And here you all go
20 back to your respective institutions and
21 keep doing the good things, but what about
22 the --

1 MR. FREEMAN: Well, it's
2 suboptimal, I understand. I admit that.
3 The political methodology group does not
4 have problems getting grad students to come
5 to meetings. Our problem now is that we're
6 like a traveling circus. I mean we get,
7 like, 135-40 people, and out of that, what,
8 60 or 70 are graduate students?

9 MS. MORTON: Yes, in fact we
10 had -- I think the problem is getting
11 faculty, right? I mean, wouldn't you say --

12 MR. FREEMAN: And also making
13 people feel appreciated, making them feel
14 that they're not being processed, that they
15 can have the poster sessions and they really
16 get to meet us and we talk to them and we
17 stick around and we spend time with them,
18 and they're not just sitting in a big room
19 instead of around a table.

20 Like at —, we used to have 21
21 people, everybody had to present. Now we
22 have this huge auditorium and -- that's the

1 problem, to personalize it.

2 But I don't think in the realm of
3 political methodology there's any problem at
4 all. I mean, I think getting 80 to 120 grad
5 students is easy.

6 MR. ACHEN: And that adventure
7 from its beginning shows what can be done.
8 There were fewer people around the table at
9 the first meeting of that group, which
10 wasn't funded by anybody except Warren
11 Miller basically, than there are around this
12 table. There were 14 people at the first
13 meeting and now it's well over 100 and you
14 can't chat with everybody anymore and it's
15 changed.

16 So it's, I think, doable. This
17 one is much larger and more complicated. I
18 think I have to say, I think in both poly
19 sci, NSF, and MMS, as a little bit starved
20 for money by international standards. And I
21 think this will take some money. A few
22 years ago, when MacArthur wanted to bring a

1 group of people into -- it's about 12 or 15
2 years ago -- into international relations,
3 for example, they bought people's time for a
4 year, for instance, quite a large number of
5 people. And it made a huge difference.

6 The same thing is happening with
7 Robert Wood Johnson with these medical care
8 fellowships. They are building a foundation
9 for the future. But they're buying people's
10 time for 2 years in pretty substantial
11 numbers.

12 So I guess one question I have
13 here is who we have to -- I shouldn't say
14 "we" -- do you have the resources to really
15 make a substantial difference on this?
16 Because I think some real training and time
17 off and all that kind of thing, some setting
18 up of institutes. Those things aren't free.
19 Doing that is what will speed this whole
20 process up.

21 MR. FREEMAN: — 5-year graduate
22 package, MacArthur pays 2 out of the 5

1 years. So that's about \$45,000 per student.
2 And then they have ——. And I guess I've
3 got to tack this on. I know this is public
4 and I'm going to regret it, but believe me,
5 the MacArthur people at Minnesota are not
6 teaching scientific method. They are
7 exactly the opposite. And I've been to
8 Stanford for them a couple of -- I've
9 stopped going. That movie, "My Dinner With
10 Andre," have any of you seen that? Need I
11 say more? It's fascinating, it's rich, it's
12 diverse, it's the celebration of life -- but
13 it ain't ——.

14 And that's if we're in
15 competition. I mean, a scarce number of
16 people for whom the large funding industries
17 in this country are competing for their
18 hearts and minds. God, I'm really getting
19 in trouble now. Quite seriously, I mean,
20 there's a very small number of students and
21 they're being courted by all these funding
22 agencies. And they're not all sharing the

1 same objectives. Even though -- verbally
2 they are, and the rhetoric is there -- the
3 reality's quite —.

4 MS. MORTON: One thing related to
5 that and to the international -- going back
6 to international relations, it does seem to
7 me that of the fields, that IR is the most
8 divided in terms of having a substantial
9 group of people really anti-theory. I mean
10 in a scientific sense.

11 MS. ZINNES: Theory or
12 anti-modeling?

13 MS. MORTON: Anti-modeling.
14 Anti-science. I mean, it seems to me there
15 are more of that, but I don't know -- you
16 know, that just seems to me, you know --
17 because when I talk to our IR people, they
18 seem to think that there are the same number
19 of people in American. I say, no, we don't
20 have people like that in American. The way
21 I hear about them in IR -- really anti --
22 that they're, you know, just really anti

1 the --

2 MS. ZINNES: Anti-science
3 altogether.

4 MS. MORTON: Yes. Yes. Yes. And
5 I mean it seems to me that there is a
6 special need in IR, you know.

7 MS. ZINNES: Well, it may be, and
8 I don't see that largely -- I mean, I did a
9 survey of the major journals over a 10-year
10 period and I was very surprised to discover
11 the extent to which what's called
12 quantitative, whatever that means,
13 statistical, mathematical, et cetera,
14 dominates the IR articles.

15 Now, how many IR articles get into
16 things like JOP or something -- nevertheless
17 perhaps the divisions are much more severe
18 in terms of those that don't do that, but
19 they're not the ones that are really -- the
20 ones who are objecting to that are not the
21 ones that are really getting published out
22 there. So I suspect it will take care of

1 itself over the long run.

2 MR. KEECH: In American there's no
3 good reason for American political
4 development to be anti-scientific, and many
5 people who do that are people we respect and
6 admire. However, I think that is a movement
7 or setting for people who don't agree with
8 the kinds of analytical approaches that are
9 common among us. And I think it's growing.
10 People like Stuart and Weingast also publish
11 in studies in American political development
12 and make my point that there's no reason
13 that the subject matter has to be
14 anti-analytical.

15 MR. SCIOLI: All right, let me not
16 dodge Chris's question, but say that it's
17 too early in the day to get to a hard answer
18 or even a squishy answer. But the fact that
19 Cheryl's here -- you've observed that.

20 At the beginning of the morning
21 Norman Bradburn mentioned the priority
22 areas. And our job as program officers is

1 to try to include as much of this topic
2 within that priority area when it comes to
3 fruition. So we're thinking about a number
4 of ways in which we might make progress, but
5 we're certainly only at the beginning stages
6 in that.

7 MR. GRANATO: Why don't we move on
8 and talk about methodology in modeling, and
9 start with Chris.

10 One more thing, too. Discussion
11 point 2, discussion point 4 are related to
12 the discussion we just had for the last 20
13 minutes. We're going to revisit these
14 concerns again. Chris.

15 MR. ACHEN: I'll be brief. There
16 are three of us in this category who want to
17 talk.

18 Let me just say that it seems to
19 me that we have quite a bit of thinking to
20 do on exactly how empirical work broadly
21 conceived does connect to validation of
22 theories. And I talked a little bit about

1 this in the paper I wrote. And I won't
2 repeat, except to say that I do think that
3 our current training, the training that's
4 been current now since I was a graduate
5 student -- so maybe not so current, but it
6 hasn't changed; we do it exactly the same
7 way we did it when I was a student -- isn't
8 as helpful as it might be and is no longer,
9 I think, the training that people need.

10 But I think we do want to spend
11 some time here thinking about the incentives
12 that we have, and students have at both the
13 graduate and the undergraduate level, to do
14 things the way we do them now. Let me start
15 with the undergrads, where the point is the
16 most obvious.

17 Faculty appointments are driven at
18 most places by the size of the undergraduate
19 enrollment. If you turn political science
20 into what the people around this table would
21 want it to be turned into, the enrollments
22 would drop, budgets would drop, appointments

1 would drop, a lot of other things would
2 happen. And I think we all try hard at
3 every institution I'm aware of to do what we
4 think is right and pay as little attention
5 to these other consequences as possible.
6 But if you've been at it as long as all of
7 us in this room have, at the margin you see
8 some decisions being made that simply
9 protect those budgets.

10 So I think we've got to think
11 about ways in which the undergraduates who,
12 you know, are on their way to law school and
13 who will say to you things like one of my
14 undergraduates from one of the most
15 prosperous Michigan suburbs said to me at
16 office hours last fall, when I was teaching
17 difference equation models -- came in and
18 she said I'm having a lot of trouble with
19 this, I don't see this point here. And we
20 spent 15 minutes going over it. And I
21 finally said, I think what you're not seeing
22 is that if $A = B$ and if $B = C$,

1 then A ought to be equal to C. And she
2 looked at me with a sad look on her face and
3 she said, you know, my mind just doesn't
4 work that way.

5 We have a fair number of those.

6 MS. ZINNES: But she's a high GRE,
7 right?

8 MR. ACHEN: Well, she may be. So
9 I think we have to think about them, too,
10 and how exactly they would fit into what we
11 want to do.

12 At the graduate level the problem
13 is somewhat different but it's related, and
14 that's the area studies people, their quite
15 separate agenda. I don't think it's an
16 accident that in most political science
17 departments now the main cleavage is area
18 studies versus the rest of the department.
19 And there again, there are incentives there
20 that are quite different from those that we
21 face.

22 So how does the topic of this

1 workshop play into all of these concerns? I
2 think it plays in mostly by bringing to our
3 attention the fact that we do have these
4 colleagues back home, we do have to think
5 about ways in which things can be
6 restructured in ways that will create the
7 right set of incentives for people.

8 We're redoing our undergraduate
9 curriculum right now and one thing we're
10 trying to do is to give a separate honors
11 degree in political science, and this
12 separate honors degree will be in fact what
13 we're talking about here, that leaves the
14 other people in place and maybe creates some
15 incentives to do some other things.

16 But I think it's this kind of
17 working out of a graduate and undergraduate
18 curriculum in ways that's really politically
19 feasible given the set of people we have,
20 who have agendas quite different from those
21 of us around this table. That's a pretty
22 important thing to do.

1 As part of that, I do think -- and
2 I'll just say this briefly -- I do think
3 there's a deep and hard rethinking that
4 needs to go on about the ways in which
5 empirical evidence is brought to bear on
6 theory. And Dina just talked about this and
7 so have many of the rest of you.

8 I think increasingly we see
9 econometric methods, statistical methods
10 taught to students in ways that maybe make
11 us happy, but from the students' point of
12 view -- again, they're out there drowning.
13 The Titanic has gone down and they're in the
14 water. And it's cold and the sharks are
15 circling and so on. And that's how they
16 think about all of these methods. And
17 they're desperate for a lifesaver, or a
18 lifeboat. And the lifeboat is mechanical
19 application of these techniques so that they
20 can be protected from criticism.

21 One of my friends in graduate
22 school said that most students' relationship

1 to what you do -- he was talking to me -- is
2 that of primitive people to the local
3 dangerous gods, you know, bad things can
4 happen to you if you screw up. So you need
5 to offer sacrifices, you know, t-test and so
6 on.

7 That really is the way a lot of
8 people respond. I think the curriculum
9 plays into that more than it should, and
10 that some rethinking of the ways in which we
11 really can focus when the puzzles and the
12 scientific bottlenecks, whatever word you
13 want, is in front of us. But the
14 institutional momentum, for some of the
15 reasons I've mentioned, in the textbooks and
16 so on is pretty substantial.

17 I think this will work itself out
18 if left alone. If we want it speeded up, I
19 think it's not a \$1.95 budget that will do
20 that.

21 MR. GRANATO: Henry.

22 MR. BRADY: I want to start by

1 thanking Jim and Frank for inviting us here.
2 This is really a great opportunity, and it's
3 tremendous to be able to sit around the
4 table with this group of people. I'm also
5 pleased to be able to follow Chris Achen
6 once again, which I've been doing for, I
7 think, 22 years -- mostly with good effect,
8 I think. But it's always great to hear him
9 talk and to read what he has to say.

10 I'm not going to repeat what I
11 said in my comments. You can read it. It
12 quotes philosophers and things like that in
13 an attempt to try to sound erudite, and to
14 actually provide some background to why we
15 are here, why there is a difference between
16 people who think in terms of formal models
17 and in terms of empirical work. And it
18 really is a deep division.

19 I want to just make a few comments
20 about that and then go on and talk about
21 where we should be going. The few comments
22 are the following, is that it seems to me

1 that one of the differences between formal
2 modelers and empiricists is that formal
3 modelers often think the truth is hidden in
4 unique models that sort of get at what's
5 really going on.

6 So, for example, in IR, formal
7 modelers are concerned very much about how
8 you get at deterrence and that it's a really
9 hidden kind of thing in there. I mean,
10 unless you have a really pretty
11 sophisticated understanding of what's going
12 on in the interactions between states,
13 you're not going to get at deterrence.
14 Whereas often empiricists think these things
15 are more superficial and you can just find
16 them in the appearance of things.

17 I tend to be on the formal
18 modeling side in this regard. I think in a
19 lot of cases what formal modeling has tended
20 to do is help improve our understanding of
21 concepts. Oddly enough, I'm not sure formal
22 modelers often realize how important that is

1 as an aspect of what they do. And one thing
2 I think, more could be done on that to make
3 sure that people who do empirical work
4 understand how important it is to have
5 formal models often to just clarify the
6 concepts you're using. And that is a very
7 important thing.

8 I've run this idea by Bob Paul (?)
9 and he gets upset. He says, oh, no, we do
10 much, much more than that. And I say, well,
11 yes, maybe you do, but I think that's an
12 important thing to focus on is how much do
13 just clarify concepts and that would help
14 empirical work a lot.

15 Let me just talk about the two
16 techniques, that it seems to me the
17 perversion of mathematical formal modeling
18 is that you get theorems that are sort of
19 meaningless; the perversion of statistical
20 work is we get estimators that are
21 meaningless in terms of real empirical work.
22 I feel a little guilty about this because,

1 following Achen, I helped to start that
2 political methodology group. And sort of
3 the currency of the realm there is new
4 estimators, and that's supposed to be very
5 exciting.

6 I don't think that actually was
7 quite Chris's message from the beginning,
8 and certainly now, if you read what he says
9 in his marvelous little paper of his, to
10 some extent, maybe to a large extent, a
11 rejection of that idea. And then there's
12 the following marvelous statement: "I
13 propose the following simple rule: Any
14 statistical specification with more than
15 three independent variables should be
16 disregarded as meaningless." I'm not sure
17 how you got to three. But there's a lot of
18 truth to that. I think we've got to think
19 in methodology a lot harder about what we've
20 been teaching people. And I feel even
21 teaching them with sophisticated maximum
22 likelihood or whatever kind of estimators

1 are the answers to our problems, when in
2 fact that's just not the case at all.

3 We need better research design.

4 We need to think harder about how you make
5 an argument with nature and deal with its
6 cunning wiles to get at the truth. And I
7 don't think we teach people enough about
8 that, how hard it really is to make any
9 argument, how hard it is to infer something.
10 And that t-statistics, good estimators,——
11 consistent standard errors are not
12 necessarily the way to go.

13 I think if we did more of this,
14 then I think there'd be a more natural link
15 with formal modeling, because pretty soon
16 we'd realize the problem is just clarifying
17 our concepts, clarifying what we're really
18 trying to get out, and we'd say, gee, formal
19 models can be very useful in that
20 enterprise. And to the degree that we hide
21 behind statistical technique, I think we put
22 off the formal modelers in bad ways. And so

1 that's something that really has to be done,
2 is to think harder about what we're trying
3 to teach people in political methodology.

4 Let me just say something about
5 the caricatures that each side has of the
6 other. It seems to me that the caricature
7 that formal modelers often have of
8 empiricists is that data can't tell you
9 anything, and basically the argument is that
10 it's the problem of induction --
11 association, not causation. And I think
12 there's a lot of truth to that problem.
13 Again, formal models can often help show you
14 how you can rule out alternative
15 explanations. So that can be a good thing.

16 At the same time, I think that
17 the -- and I've heard formal modelers really
18 say this, you can't really learn anything
19 from data very much because it's so
20 terrible. And one approach to that is to do
21 what Dick's suggesting, is to take refuge in
22 just doing experiments. I think there's a

1 lot of good reasons to do experiments, but
2 I'd be nervous if that's the only direction
3 we went in terms of empirical work.

4 So that's one caricature. On the
5 other side, I think that, and people have
6 said this already, formal modelers are often
7 characterized as models can't tell you
8 anything; after all, they're simply
9 tautological -- which of course they are if
10 they're done correctly. But that's wrong,
11 too, for the reasons we all know. So we've
12 got to get over those stereotypes.

13 And some ways we might do this.
14 Well, it seems to me, as I mentioned in my
15 memo, behavioral economics is one way that's
16 helping to bring things together.

17 Natural experiments. One of the
18 things that's really happening in
19 methodological work, especially in
20 economics -- I would hope more in political
21 science as well -- is we're beginning to
22 realize that we have to go around and look

1 for observational data where there have been
2 natural experiments. Because we really
3 can't have much faith in being able to
4 specify complicated systems of equations and
5 getting much out of that unless we have
6 really good instruments, which often just
7 amounts to saying there's a natural
8 experiment in there someplace that will
9 allow us to do that. Better research
10 design. I think there's more realization
11 we've got to do better in that regard. And
12 as I say, I think that will lead to a
13 natural bridge.

14 How do we do these things? I
15 think -- there's been a lot of suggestions.
16 I just wrote down 20 suggestions that come
17 from the memos. I'll not go over them, but
18 I hope one of the things we do in the
19 succeeding sessions is write all these
20 things down. Maybe we need a board, like
21 over there -- I'll assume you're going to
22 maybe do that -- and put them up. And as we

1 go through them, we can say, well, which
2 ones really would work? And they've got to
3 be not just suggestions in terms of
4 procedures but also in terms of substance.

5 So, for example, a substance
6 suggestion would be tell people that one way
7 they can talk to comparativists is talk
8 about macro concepts, so macro modeling
9 might be one way to do that. In terms of
10 process it might be let's have summer camps
11 where we bring people together to talk about
12 these things. And I hope we come up with
13 both kinds of suggestions.

14 MR. GRANATO: We're running up
15 against the clock. Would you like to start
16 after lunch, or --

17 MR. McKELVEY: Yes, I guess I'd
18 prefer that.

19 MR. GRANATO: Okay. All right, so
20 let's break for lunch.

21 (Whereupon a luncheon recess was
22 taken.)

1 A F T E R N O O N S E S S I O N

2 MR. SCIOLI: We've been joined by
3 a colleague from social and economic
4 science. Paul Wahlbeck is program director
5 for law and social science, visiting
6 scientist from George Washington University,
7 just back from a meeting in Budapest.

8 I guess one of the unintended
9 positive consequences is that you folks know
10 each other better than 90 percent of the
11 people we bring into rooms like this. So
12 it's harder to get you to stop talking at
13 the breaks and, geez, the luncheon
14 discussion here was -- I think we have to
15 include some of the formal people, some of
16 the empirical people, and then some of the
17 ascientific people, and that would probably
18 cause all the discussion at the breaks to
19 stop. Right, Henry?

20 MR. BRADY: Yes, either that or
21 you get fist fights.

22 MR. SIMON: What do you mean, fist

1 fights?

2 MR. BRADY: Outside, big guy.

3 MR. SCIOLI: Well, we've been
4 waiting for the senior members here to
5 return, those who taught -- who I thought
6 were the senior members.

7 MR. McKELVEY: Yes, well, first
8 thing I was going to say is I feel very old.

9 But the second thing I've noticed
10 is that seem to be a lot more insulated from
11 the things that we've been talking about
12 than most of the other people around this
13 room. Part of it, I guess, is just because,
14 being at Cal Tech, it's not a standard
15 political science department so you'll
16 interact mainly with economists and the
17 other political scientists that are on our
18 faculty, who mainly sort of share the views
19 of most of the people here.

20 So maybe one of the ways of
21 addressing this divide would be to take
22 people like me and other people at Cal Tech

1 and place them in the universities that you
2 guys are from. But -- I'm not seriously
3 suggesting that.

4 Anyhow, I guess -- yes, so let's
5 see. I guess the first thing I wanted to
6 say something about was just give a little
7 bit of an overview of what I think has been
8 sort of the main direction that political
9 theory has gone over the past several
10 decades. And I think actually this
11 direction has contributed to some of this
12 drift that we see between empirical and
13 theoretical work.

14 I mentioned some of this in my
15 comments that I distributed. But my
16 question is that as opposed to economics,
17 where the far Asian market economy is taken
18 as sort of a starting point for a lot of --
19 or at least it was for a long time, I mean
20 it's less so now, but for a long time it was
21 taken as a starting point everyone could
22 agree to. So then when you go off and do

1 empirical analysis, everyone had something
2 that they agreed on, that they could start
3 from.

4 In political science, we've never
5 really had that, at least not since Charlie
6 Plott — and Anthony Downs, and a lot of
7 the other work on sort of lack of equilibria
8 in these political models led us to the
9 realization that you really need to model
10 institutions.

11 And so I think what's happened
12 over the past several decades in the
13 theoretical work is that there's been an
14 attempt to bring in the details of what
15 you're modeling. And you're sort of averse
16 to do that because there is no sort of
17 general equilibrium theory.

18 What this had meant is that we've
19 tended to -- well, first of all, we've
20 started to model, I guess, incomplete
21 information, repetition, as I said, the
22 details of these institutions. And so a lot

1 of the models require that you specify, say,
2 an extensive form for a particular order in
3 which people make decisions and so on. And
4 so these are areas in which theorists
5 sometimes I think just throw up their hands
6 because you have to throw in too many
7 particularized details. You can't get
8 really general results.

9 If there are any general results
10 that will come out of this, I think it's
11 that -- well, some of the results, say, of,
12 like, Peterson, Peserdorfer, Banks, Austin
13 Smith, these results on what happens when
14 you introduce incomplete information. They
15 just show that you get very different kinds
16 of results than what people thought you
17 would get in the complete information
18 models.

19 And so on one hand we see how
20 important these variables are, but on the
21 other hand I don't think we're at the stage
22 where we have any kind of general theory

1 about how these kinds of variables affect
2 things.

3 On the other hand, I think one
4 thing that theorists do agree on is the
5 importance of game theory. But even here,
6 there's a little bit of a qualification.
7 Certainly most theorists, I think, accept
8 the idea that any kind of theorizing has to
9 start from a game theoretic basis. And
10 here, I guess the way I convince my students
11 of this is I just tell them, look, if you
12 want what you publish to be still read 20
13 years from now, it's going to have to be the
14 case that your description of behavior is
15 still going to be relevant after people have
16 read it. Okay, and so the very definition
17 of Nash equilibrium is that people will not
18 change their behavior once they've read the
19 theory.

20 So I think this sort of
21 encapsulates the idea of why these game
22 theoretic models have really sort of taken

1 over, both in economics and political
2 science, where we're trying to explain human
3 behavior. You want an explanation which is
4 still going to work even after you tell
5 people what the explanation is, what they're
6 supposed to be doing.

7 So we sort of agree that these
8 models have to be game theoretic, but beyond
9 that I think the most we agree on is that
10 you've got to have really detailed
11 information about the particular process it
12 is that you're modeling. Now, on the other
13 hand, I think this is sort of problematic
14 from the point of view of getting any
15 general theory, but I think it also provides
16 us opportunity in which there's really a
17 chance for the empirical end of the spectrum
18 and the theoretical to have some common
19 interest. Because I think it's the
20 empirical people who -- they know a lot more
21 about these details and they're more
22 interested in them.

1 They don't particularly care when
2 theory is general, and so they're frequently
3 a lot more willing than the theorists to
4 develop a model that's based on particular
5 details of the processes they're studying.

6 So this is one of the reasons why
7 I think this effort to bridge this gap is a
8 good idea, because I think there really is a
9 niche there for people who can speak to
10 theorists and speak to empirical people and
11 sort of do some of both.

12 I think -- let's see. One of the
13 other comments I made is in -- what I tossed
14 out was that right now, at least myself, I
15 frequently find that a lot of the empirical
16 work, what I do read, I guess I frequently
17 discount it because -- simply because of all
18 the problems that are inherent in doing a
19 good piece of theoretical work.

20 For instance, in American
21 politics, which I guess people sort of
22 agreed was one of the areas where theory is

1 most developed, I mean here, you in this
2 work, you know, you see a lot the models be
3 the theory that -- well, the theory that is
4 used is usually very oversimplified from
5 what I guess a theorist would want you to
6 look at, so frequently you look at these
7 one-dimensional models where individuals or
8 committees or legislatures are considered as
9 individual actors. And the same thing in
10 the case of IR and comparative.

11 You know, frequently the theory
12 that is considered good theory itself is
13 really simplified. And so you have these
14 oversimplified theories which don't really
15 -- they may not take into account things
16 like incomplete information, repetition and
17 so on, and they're built to explain certain
18 kinds of phenomena.

19 So one of the things that I worry
20 about in the empirical work that's done now
21 is that, you know, you can build a theory to
22 explain just about anything. So you build a

1 little game theoretic model which will
2 explain a certain kind of behavior, but you
3 could build another model that was more --
4 you know, that would introduce more features
5 of a situation, which would explain exactly
6 the same thing.

7 And I guess what's missing is what
8 we expect of theory, which is namely that
9 the theory -- you should be able to take a
10 similar theory and apply it in a number of
11 different situations.

12 So on one hand -- okay, what I
13 just said is that we want -- you know, I
14 think it would be good if some of the
15 empirical people would start helping to
16 develop some of these theories specifically
17 applied to the problem, but I think also
18 it's necessary to keep in mind that you
19 don't just develop the minimal theory to
20 explain what it is that the data shows, but
21 that we try to develop these theories, first
22 of all so that they actually reflect the

1 variables that are active in the particular
2 situation so that we can take these models
3 and explain more than just one phenomenon
4 with them.

5 Okay, and finally, I guess the
6 other reason why I tend to frequently maybe
7 not pay as much attention to some of the
8 empirical work as I should is that a lot of
9 the theoretical work that's done in these
10 empirical papers is basically game
11 theoretic. And yet the methods to test the
12 theory are still methods that are based on
13 non-game theoretic reasoning. And that's
14 starting to change.

15 There are some papers now that
16 have started to look at some of these
17 questions. But that's something that
18 actually I've been very interested in in
19 work that I've been working on with Tom
20 Palfrey is how to test some of these
21 game-theoretic models statistically.

22 But I think -- yes, that's another

1 area where there could be a bridge between
2 the empirical people and some of the
3 theoretical people, is trying to develop
4 some of the methodological tools to actually
5 test some of these game-theoretic models.

6 MR. GRANATO: Let's open it up and
7 talk about this.

8 MR. SIMON: Actually, it's pretty
9 telling, I think, this statement that you
10 just made, Richard, that there's this
11 disconnect between the game theory that
12 permeates political science theory and the
13 empirical methods that are used. But it's a
14 little scary that only you and Palfrey are
15 the ones who know about the --

16 MR. McKELVEY: No, I don't think
17 that --

18 MR. SIMON: Well, almost the only,
19 right? I mean, it's not a well-known
20 empirical technology to test the game theory
21 models that are so important for political
22 science.

1 MS. MORTON: It's been discussed
2 at the methods meetings. In fact actually I
3 would argue that the very first
4 non-experimental use of quantum response was
5 in political science with Curt Signorino's
6 paper, right? I mean, that was -- so I
7 think that political science used quantum
8 response with non-experimental data before
9 economics did. That said, there aren't that
10 many. There aren't that many in economics
11 either that use structural estimation,
12 right?

13 MR. SIMON: Actually, I will talk
14 about it when I --

15 MR. McKELVEY: But I think in
16 economics -- you did have econometrics that
17 was developed for the theory, and that's
18 where simultaneous equation estimation came
19 from.

20 MS. MORTON: Right.

21 MR. McKELVEY: Basically to test
22 the, you know, sort of the — model, sort

1 of supply and demand so on. But now -- I
2 mean actually, I think even economics has
3 moved more in the direction of introducing
4 all these specific institutional variables,
5 — information and so on, so that the —
6 model is less well --

7 MR. FREEMAN: What do you say to
8 the problem of observational equivalence,
9 that what we do in political methodology is
10 we study reduced forms because in fact
11 there's many different professional choice
12 models that could rationalize our results
13 and it's a waste of time to try to
14 identify -- Jim alluded to this in his
15 opening remarks -- to identify the so-called
16 true model that gave rise -- or set of
17 models that conceivably could give rise to
18 —?

19 MR. McKELVEY: Yes, well, I mean
20 there is no true model, right? I mean any
21 model that we -- you know, like in physics,
22 you take the Newton's -- you know, the

1 mechanics, of classical mechanics. I mean
2 there it's wrong, but it's still very useful
3 in certain classes of -- as approximation.
4 But the thing that makes a model a good
5 model is how many different things it
6 explains at the same time. You know, I
7 think that's --

8 MR. BRADY: I think he's asking
9 about the harder question, which is sort of
10 a Lucas critique kind of question, which is
11 that the truth is there are so many models
12 we might build which could lead to the same
13 set of data that it's foolish to try to
14 figure out from the data which model is
15 correct. It can't be done.

16 I mean that's the sort of base of
17 the Simms Lucas -- isn't that what you're
18 alluding to -- sort of macroeconomics and --

19 MR. FREEMAN: The Nash equilibrium
20 point as I understand it is in answer to the
21 Lucas critique, right? You're just saying
22 that the model would change. Once you saw

1 the results you would change your behavior
2 and therefore the model underlying would
3 change. When you're saying if it's a Nash
4 equilibrium --

5 MR. McKELVEY: If it's a Nash
6 equilibrium, then --

7 MR. FREEMAN: And you saw the
8 results --

9 MR. McKELVEY: Right.

10 MR. FREEMAN: But suppose it were
11 all Nash equilibrium, isn't it still --
12 isn't there still a problem of observational
13 equivalence that is in some sense
14 insurmountable?

15 MR. McKELVEY: You mean that there
16 are several different theoretical models
17 that could be built to explain the same
18 phenomena?

19 MR. FREEMAN: Yes --

20 MR. McKELVEY: That's basically
21 what I was -- one of the points that I guess
22 I was making is that if an empirical person

1 starts to build a theory about a particular
2 situation, they frequently, I think, just
3 stop when they get the result that they
4 want. Which is -- you know, that's not
5 really very much help. You want a theory
6 that is going to have a broad applicability
7 and be able to explain a large class of
8 phenomena at the same time. And that's --

9 But here I think the problem is
10 that, yes, there's really partly a problem
11 in theory right now. We don't have any
12 really -- I mean the theory we agree on is
13 game theory. At least that's what most
14 people agree on. And even there, I think
15 there's -- people explain these evolutionary
16 models and -- may not agree that that's the
17 basis for scientific theory.

18 But I think most people, I've
19 found, at least when you present them with
20 this argument that the theory's got to
21 survive its own publication, that they tend
22 to agree that the game theoretic models are

1 what we're going to be based on -- what we
2 want the theory to be based on. But even
3 so, if, you know, what -- the direction ——
4 say you have to include all these
5 institutional details because there is no
6 equilibrium otherwise.

7 MR. FREEMAN: —— indeterminacy
8 result and you run into Folk (?) theorems
9 and you run into multi-equilibrium, so you
10 take your chaos that you gave us, the
11 majority rule, and you just reproduce it in
12 the form of time inconsistency of optimal
13 plans, you know, every strategy is a
14 question of ——. You know, I find, sort
15 of, I'm left standing on a slippery slope
16 every time after I read in this.

17 The more I read, the more
18 disheartened I become. There's no order.
19 Everything's chaotic. The world's
20 unraveling. So I go back to my vector auto
21 regressions in the field, you know, ——.

22 I'm not being facetious, Richard.

1 I sometimes wonder if the real contribution
2 of you and Errol (?) and other people has
3 been to convince us that democracy's
4 impossible -- it should not be happening, it
5 should be no order. Everything is -- I
6 mean, that's a deep insight, God only knows,
7 but --

8 MR. McKELVEY: Well, no, I think
9 democracy depends on not having equilibrium.
10 If you have an equilibrium, there'd be
11 nothing for legislatures to do.

12 MR. ALDRICH: I think — argues
13 that at times; at other times —. But
14 this is essentially -- I mean your argument
15 is essentially there is an impossibility
16 result of building -- in your models?

17 MR. McKELVEY: I don't think I
18 really believe that. I just think that
19 we're in a stage in theoretical development
20 where there are a lot of things we don't
21 really understand, like, you know, how
22 incomplete information plays out in these

1 models, how repetition plays out. And, you
2 know, so that in a way, a lot -- I think a
3 lot of the problems are that theorists don't
4 have any overall theories that they can hand
5 to the empirical people and say, yes, this
6 is what you should use. Yes, I mean, I
7 think that's part of the problem. But --

8 MS. MORTON: I would like to pick
9 on something that I think was mentioned
10 before lunch. I can't remember which of you
11 guys had said this, but -- about natural
12 experiments, in that, you know, I can't
13 stand that, so -- Jim knows this already --
14 because I just -- I mean everything, there's
15 always some variable that's in the real
16 world, right? And so it's just that we
17 happened to find one that we liked the way
18 it varied and it happens to answer some
19 questions we want. But I don't see that
20 that's any more in a, you know, particularly
21 interest, you know, natural experiment than
22 any other.

1 I think that that's a nice
2 direction, because you can -- you get so
3 many results with that, but it's a real
4 problem if we build too much on that,
5 because if you come too much strapped in to
6 what actually occurs, you lose the ability
7 to think outside of the real world, and the
8 counter-factuals and the things that can
9 actually occur.

10 And then there's a lot of things
11 that we need to be able to think
12 theoretically about that we don't expect the
13 world to give us variation in the data, and
14 maybe we don't want it to until we think
15 about it for awhile.

16 I also think this is one way that
17 laboratory experiments can be very useful
18 in, for instance, like checking out
19 cumulative voting. I mean, instead of
20 having some elections where we have
21 cumulative voting and we actually elect
22 people, I mean, it's kind of nice to check

1 it out in the lab, see how it works in the
2 lab, see what kind of outcomes you get. And
3 we can't go around waiting for these
4 natural --

5 Yes, I think that this idea of
6 natural experiments as being, you know, a
7 clever way to think about the empirical
8 world, and data gives us - - you know, it's
9 nice, but I'm afraid that the discipline has
10 just gone way overboard on this and I think
11 we need to think more carefully about what
12 we're advocating --

13 MR. BRADY: Well, I want to
14 differentiate first between quasi
15 experiments and natural experiments. Quasi
16 experiments is sort of an unfortunate term.
17 I think even Donald Campbell towards the end
18 of his life wanted to recant it, because it
19 seems to suggest that observational data is
20 sort of like of experiments, when typically
21 it's not.

22 But natural experiments, that's

1 something like the draft lottery in the late
2 '60s, where you basically ratably assigned
3 people whether or not they were going to go
4 into the military, and from that you can
5 learn a lot, what happens to those people in
6 later life. That's a circumstance where I
7 don't think we could learn in a laboratory
8 what would happen to people if you randomly
9 assigned them to military service. But we
10 can, with the draft lottery, experiment.

11 In places where we can learn in a
12 laboratory, because we can manipulate things
13 in practice and ethically, then great, let's
14 do it. But in cases where we can't do that,
15 like the lottery experiment, then natural
16 experiments are a great boon because they
17 overcome the impossibility otherwise of
18 really not consigning variables. I mean,
19 you really have orthogonality between that
20 and the — actually not quite, it turns
21 out in that experiment, but that's another
22 story -- in that natural experiment, but at

1 least it's better than what we usually get.

2 MR. ALDRICH: I have a couple of
3 questions. But first I could preface by
4 saying that Dick taught me when he was a
5 graduate student and I was a graduate
6 student, and the natural experiment of the
7 lottery explains why Chris could be my
8 professor and I could be the graduate
9 student. It's not age.

10 I have two questions. I think
11 there's a couple of people asserting this
12 issue's — to be better, which -- I mean,
13 that's probably a truism that, you know, has
14 been around for a very long time. Is there
15 something new that we're talking about, or
16 is it, you know, we need to improve our
17 introductory course and make sure it's
18 actually followed through upon?

19 The question specifically for
20 Chris, if there are -- if you can't trust
21 any equation with more than three
22 variables -- are you getting up? Is that

1 a --

2 SPEAKER: He's been on a diet.

3 MR. ACHEN: After the ribs at
4 lunch -- No, I don't think so, but I do
5 think that we're not very careful with our
6 empirical work. And so even when there are
7 a dozen articles on a given topic in the
8 discipline, I often find reading them
9 through, actually in the same way maybe
10 that -- as you read them, and you think,
11 well, I don't know what to believe.

12 And part of that is just because
13 the very patient, careful data analysis that
14 I think is necessary to validate the
15 statistical models that we do use, which
16 have a very large number of assumptions
17 built into them, is rarely done. And the
18 result of that, then, is that you look at
19 the work and you say I don't really know
20 whether that inference is right or not.

21 And that, it seems to me, ——. A
22 lot of hours go into this, a lot of time and

1 effort. But the kind of patient work that
2 you see in some other fields, where a 5- or
3 10- year project will be carried out to find
4 out whether there really is little bugs in
5 the water that cause this particular
6 disease. We don't do that. We just kind of
7 run regressions and, well, they had six bugs
8 per cubic inch and they had three, and sure
9 enough there was more diphtheria in Area A.

10 It sounds like a caricature, but
11 it isn't. It really isn't very far from
12 what we do. And I do that careful selection
13 of subsamples will allow you often to get
14 rid of six or seven of these variables
15 because you really controlled for them with
16 the sample that you picked and the people
17 that you talked to in the survey and so
18 forth. And the result of that would be, you
19 know, he says 5-3; well, I counted the
20 number of dimensions I live in. You can
21 look at 3-dimensional data on a screen and
22 see what's going on.

1 And that is the difference, I
2 think. Right now, you look closely at the
3 empirical work that's in the journals, you
4 look closely at it, take time with the data,
5 give yourself a month, go through it line by
6 line, and it's a reliable event that
7 sometime over the course of that month you
8 will find hideous mis-specifications in
9 there that move everything around really
10 badly when you ——. That seems to me to be
11 on people like me. You know, we've trained
12 people a certain way and we're paying for
13 it.

14 MR. ALT: I think -- you know,
15 that's a point worth spending some time and
16 thought on, because if acted on in different
17 ways it would make some of the things we're
18 trying to fix actually worse. It would, I
19 think, in many ways increase the divide
20 between the theorists -- I'm like Becky, I
21 can't remember now who said exactly what
22 when, so forgive me for getting it wrong.

1 I think it was Henry who really
2 stressed the point about the elimination of
3 alternative explanations, as opposed to the
4 kind of, you know, have a theory, write down
5 a model, collect some bit of data, find --
6 you know, find traces of the effect you want
7 to predict in the data, and regard your job
8 as done.

9 That's right. I mean, the point
10 to me is not that that's a problem. Those
11 are different parts of the enterprise, as is
12 the kind of very careful validation of
13 statistical work that Chris is talking
14 about. My feeling is the field's a little
15 small and not nearly rich enough to do all
16 those things as well as we'd like.

17 My guess, and Bill, I'd welcome a
18 comment back from you on this, is that in
19 economics people tend to assume that that's
20 just going to come out in the wash, every
21 finding is going to be followed up by some
22 number of other people at some point and,

1 you know, eventually all relevant
2 mis-specifications will be located or they
3 won't be big enough to worry about. Because
4 it's not my impression that I --

5 MR. BUTZ: You — economists who
6 believe in an efficient method.

7 MR. BRADBURN: But that's -- isn't
8 that variable a mind state?

9 MR. ACHEN: Segue. I would say
10 that most people think that.

11 MR. ALT: Yes, and I think --
12 that's why what I -- you know, basically I
13 think Chris is right and I think what he's
14 put his finger on is that we're a much
15 smaller field, and so there are enormous
16 gaps in what actually gets checked up on.
17 And, you know, things survive for 3 years, 5
18 years, 6 years, then somebody says, you
19 know, they coded that wrong. And, you know,
20 everybody goes back and rethinks.

21 MR. YOUNG: Well, can I jump in
22 here with a remark about economics? I mean,

1 I -- there's a more -- a darker
2 interpretation, which is that since
3 economists come to these questions with a
4 received theory and the theory generally
5 predicts the direction of movement, actually
6 I think a great deal of empirical work goes
7 on that's very sloppy in economics. If it
8 gives a prediction that's in the right
9 direction according to the theory, everybody
10 just buys into it and they -- and actually
11 it may not be corrected.

12 So part of the difference, I
13 think, between the two subjects is that
14 within political science there's much more a
15 priori skepticism that the theory was right
16 to begin with, and therefore you get
17 searching critiques of attempted empirical
18 estimation.

19 But agreed that it's a small
20 field, and of course it's difficult to have
21 the proper number of follow-ups and
22 embellishments and so forth.

1 MR. ALT: Yes, I actually think
2 that raises another good related point that
3 I talked on before at other meetings down
4 here. I think it's actually -- in political
5 science, let's go back to the previous case
6 where you have a theorist who derives one
7 implication, finds it in the data, and says
8 job done. And then a lot of, you know, this
9 finger-pointing that says you have to check
10 it more carefully, control for all the
11 relevant variables, check your specification
12 against the kind of usual way of doing it,
13 and eliminate alternative explanations.

14 My view has always been that
15 there's absolutely no reason to ask the
16 person who did that first piece of work to
17 do those other things. It's actually
18 inefficient to do that.

19 Any theorist will, not by cause of
20 deceit or ill-nature, but just by knowing
21 what they know better than what they don't
22 know, do a better job of generating

1 imputations that are likely to support what
2 they believe than really being capable of
3 thinking out those things that are most
4 likely to falsify it.

5 So it seems to me it's healthy
6 that we're skeptical. But it should be the
7 people who believe other things who then,
8 instead of saying, eh, you know, partial
9 equilibrium, God I hate those small
10 regressions -- actually go out and do the
11 work of saying there are three other ways
12 that I think we could get to this result;
13 here are the theories, here's some data,
14 here is a result; and look, they're not
15 excluded or yes, one is excluded and one
16 isn't.

17 This would be much healthier.
18 There is in the memos -- and I want Henry to
19 -- I want you guys to give Henry that piece
20 of paper so he can write those 20 things up
21 there so we can begin kind of checking ones
22 that we like a lot.

1 But there was in one of the
2 proposals a suggestion, which I think would
3 be very valuable, to bring groups together
4 on a common problem who actually have
5 different approaches to it. It's got to be
6 handled carefully -- hard to do it so they
7 don't just talk past each other or not talk
8 to each other at all. We all know that,
9 because we've all tried it.

10 But I think that is the way to get
11 to some of the things Chris and Henry and
12 Peyton -- and everybody else, because we're
13 all agreed in a way -- are talking about. I
14 don't think it's a good idea to say, you
15 know, yet again, train one person to plan to
16 do it all because that's just not how
17 research is going to get done.

18 MR. SCIOLI: It seems like a
19 natural segue to our visiting disciplines to
20 tell us about how the divide exists. I
21 think we've heard some of Peyton's in
22 economics, but this whole -- easy stuff or

1 not, what's the big deal.

2 MR. SIMON: Well, I guess I wear
3 three other hats, so I may try and talk
4 about math, economics, and epidemiology,
5 where I have some appointments in.

6 Was it Jim who said it was a
7 problem of science, not political science.
8 And that's true in all these areas. As
9 Becky said a little while ago, economics has
10 the same problem. I did this little survey
11 of, you know, randomly chosen APSRs and
12 AERs, and while it is true that there was no
13 article in the APSR that combined theory and
14 empirics, there were very few in economics,
15 maybe 20 percent in the AER, which is a
16 rather -- the journal. If it's going to be
17 anywhere, that's where I think you might
18 find it.

19 MR. ACHEN: In 1981.

20 MR. SIMON: In -- no, in 2000.

21 MR. ACHEN: Oh, in 2000.

22 MR. SIMON: In 2000, too. There

1 was very little change. It went from 15 to
2 20 percent.

3 The -- so that it certainly -- you
4 know, the topic of this meeting is a
5 disconnect between theory -- formal theory
6 and empirical modeling. And I think it's
7 there. It's certainly there in epidemiology,
8 as I mentioned, and I won't repeat things I
9 wrote about. But NIH is a good example.
10 There's a case where the government
11 organization is pushing it, and it's very
12 difficult to squeeze theory in an NIH
13 proposal. They really do want to see
14 empirical work and almost only empirical
15 work.

16 Even true in math. I mean, math
17 the divide is between the very pure and the
18 impure applied math. And, you know, you
19 would think that a pure problem would come
20 from some real-world example or that a
21 real-world example might somehow tie in and
22 be motivated by the pure techniques, but

1 it's absolutely not true, and I'll tell a
2 little story about that shortly.

3 So I think it's there in all the
4 fields. It's certainly there in economics.
5 Economics does have a simpler task, I think,
6 than political science. We've talked about
7 that in different talks here today. You
8 know, the terms are better defined, the
9 problem's simple. I mean, microeconomics is
10 simply modeling decision -- you know,
11 decision theory, modeling decisions of
12 consumers and firms and some -- at least how
13 it's -- Peyton raised his eyes a little bit,
14 so I'll hedge on that a little bit. But the
15 way it's taught in the standard micro book
16 10 years ago, I think that it's pretty much
17 a course in decision theory, which
18 automatically makes it pretty easy to model.

19 So it has, to make it simpler,
20 it's got all this nondynamic, the
21 equilibrium, the pure rationality. And part
22 of the problem, I think, is something that

1 those of us who do genetic algorithms call
2 premature convergence. Economics has tended
3 to glump onto equilibrium notions and
4 perfect rationality, and maybe to its
5 detriment in some ways.

6 I think it has become more
7 empirical, and you can certainly see that in
8 the AER articles, where the empirical
9 part -- I mean, in 1981 there was almost no
10 empirical part, and more than half the
11 articles were empirical by 2000. So I think
12 that's worth noting.

13 I agree with -- most of it I --
14 instead of repeating what I said, I actually
15 find I'm commenting on trying to give a well
16 laid out comment on things people said. But
17 I do think in political science it seems to
18 me the problem is that the theory is so
19 poorly developed and maybe a rather a scary
20 thing to students.

21 Richard mentioned game theory as
22 the thing that -- the theory that political

1 science hangs on. Most of the theory I know
2 in game theory's pretty much 2-dimensional,
3 I mean two-person, rather. At least, you
4 know, the kind that most students learn, the
5 kind that you would teach an introductory
6 course on. It's sort of a strange political
7 science paradigm to have something that
8 works well for two people or two groups. So
9 I think there may be another problem there.

10 So I certainly would hope that
11 political science doesn't build on
12 equilibrium and pure rationality. To do
13 that it would have to open up to other
14 techniques. And I think as I mentioned in
15 my writeup, computer modeling is a good way
16 to tackle things that are not amenable to
17 paper-and-pencil calculus groups.

18 There are plenty of problems --
19 how do you prove something general out of
20 two or three computer runs? Well, you
21 can't. You have to think this out.

22 Another problem that -- I do want

1 to challenge Chris's statement about the GRE
2 score. I just can't believe that in fact
3 the political science -- there must be a big
4 tail -- maybe you're talking about the mean
5 and there's a big tail that, you know, the
6 variance may be large.

7 I also think -- the articles that
8 I've read in political science, the empirics
9 are much less careful than they are in math.
10 The best statisticians I know are some of
11 the econometricians, like Gary Solon and
12 that group. And except for Chris, who I
13 think is -- leads the charge in being
14 careful.

15 But formal modeling is still, it's
16 hostile in political science, it's hostile
17 in epidemiology, it's just very comfortable
18 in economics.

19 Bill Keech mentioned culture as a
20 variable, and I think that really highlights
21 the part that in our disciplinary approach
22 is called for to really carry this out in

1 the way that makes sense in political
2 science.

3 We talked about some suggestions
4 for fixes. I do think post-docs are a great
5 idea. They don't exist in economics.
6 Although I meant to -- I talked about --
7 economics hasn't had this discussion, as
8 clearly they should. I mean, they do a
9 little better, but not much, than political
10 science, and maybe this meeting will move
11 political science beyond economics.

12 Post-docs would be great. It
13 takes a little bit of change in expectations
14 of what happens to new PhDs. But the Robert
15 Wood Johnson foundation health scholars that
16 Chris mentioned are a fantastically
17 successful idea, of taking students out for
18 2 years, putting them in an environment that
19 is conducive to what you'd like them to do
20 and learn, and drawing them out and watching
21 the magnificent changes they make -- in this
22 case, you know, putting health policy into

1 their work.

2 I like the idea of summer
3 workshops. Scott Page teaches one at
4 Michigan in ICPSR and also one at Santa Fe
5 on computational political science. And
6 those are oversubscribed.

7 And I really think NSF can make a
8 difference. And let me close with a little
9 story. Personal background -- my PhD's in
10 math, but at some point I was 50-50 math and
11 economics at Michigan. And I really liked
12 the economists more. They were much more
13 fun. Ted Bergstrom and Hal Varian and Ken
14 Binmore -- a wonderful crazy bunch.

15 So I decided to move -- and
16 50-50's an unstable equilibrium. You know,
17 everyone expected 100 percent. So I decided
18 to move 75 econ, 25 math. And the chair of
19 the math department at the time said great
20 idea, Carl, in fact why don't you go 100
21 percent over to econ because you're an
22 applied mathematician and, you know, that's

1 not our favorite kind. I mean, he literally
2 said that.

3 And then NSF, right about a year
4 later NSF math started really emphasizing
5 the importance of applications, of having
6 math be founded in real- world problems.
7 And suddenly there was a little more
8 support. And the same person came back to
9 me 2 years later and begged me to change my
10 appointment back.

11 And I think this is purely an
12 NSF-related -- you know, that -- the
13 initiatives at NSF generated quite a big
14 role. So I think it can make a difference.

15 MR.GRANATO: Peyton.

16 MR. YOUNG: Actually, Carl, you
17 provided the perfect lead-in for me. I'll
18 also give a story related to Michigan. I
19 got my PhD in mathematics at Michigan. And
20 at the time, Harnos (?) was there and, of
21 course, just one of the most spectacular
22 teachers ever.

1 However, I found -- he was giving
2 a course in functional analysis. And after
3 about 3 weeks, we hadn't seen anything but
4 point set topology. Nothing. It was just
5 totally abstract. Not a function in sight.
6 And so I went to him in his office hours and
7 I said, you know, this is a really
8 fascinating area, but I wonder if you could
9 just give me a book, tell me about a book or
10 two that would give me some historical
11 perspective on the field. Where does all of
12 this come from? What is it good for?

13 And he looked at me and said, "I
14 don't know and I don't care."

15 So much of what I wanted to say
16 has been said. The one thing I'd like to
17 pursue a little bit that's come up in
18 several of the comments is the question
19 about whether formal theory in political
20 science really is just game theory. I don't
21 think anybody meant to say it was "just"
22 game theory, but there has been a tendency.

1 If you ask somebody in political science
2 what do you understand by "formal theory,"
3 they very often will say the rational actor
4 model or game theory or some combination
5 thereof.

6 Now, I think, frankly, that's part
7 of the problem. That's too narrow a theory
8 for political science to work with --
9 leaving aside the question of whether it's
10 right, of course. I mean, economists don't
11 bother themselves too much with that, so why
12 should we here -- I mean, what if people
13 aren't rational?

14 But leaving all of that aside,
15 there are various problems and questions,
16 central questions, in political science that
17 actually I don't think are very well
18 addressed by any of those theories.
19 Actually, Errol's (?) -- you know, and
20 earlier, Duncan Black (?) -- I mean the
21 whole theory of voting has a whole
22 incarnation that's completely separate from

1 game theory.

2 Now, you can reinterpret some of
3 this in terms of strategy and equilibrium
4 and misrepresentation and all the things we
5 know about, but the fact of the matter is
6 that, going back to the French
7 Enlightenment, the idea of a theory of
8 voting, of collective decision making that
9 had a normative flavor, that's to me also
10 eminently a theory. It's a formal theory,
11 it's a very carefully reasoned theory.

12 And I think if one cared to one
13 could make a list in political science of a
14 number of these things that are simply sort
15 of conveniently forgotten or kind of pushed
16 to the side. It's not being maybe general
17 enough to qualify as "the" theory in
18 political science, but that's where I think
19 the mistake is coming.

20 Political science addresses a huge
21 variety of problems, and I think sometimes
22 we're just going to have to admit that the

1 relevant theory isn't rational actor models,
2 it's some other theory. It's a theory
3 that's tailored for that particular problem.

4 Just to give another kind of quick
5 example that I myself have worked in, you
6 asked the question how do entities -- public
7 agencies or something -- distribute scarce
8 goods.

9 Well, you can develop a theory of
10 this. It's a partly empirically based
11 theory. It's a theory of fair division, as
12 it were, but it's an empirically based
13 theory. You can develop terms of reference,
14 you can go out and study whether these terms
15 are or are not in practice. You can do the
16 whole thing without a single mention of the
17 word "rationality" or a single mention of
18 the word "game."

19 Now, I'm not saying it's
20 contradictory to those theories. What I am
21 urging is that a broader definition of
22 theory -- that we think about enlarging the

1 boundaries of theory in political science.
2 And indeed, I think this is one of the great
3 advantages of political science. I've
4 always been envious of your guys. I mean,
5 you know, economics is so trapped, in a way,
6 in a narrow explanation of human behavior.
7 It's just hopeless. But we'll never undo
8 it.

9 You are in a position where
10 heterodoxy reigns, or at least it did reign
11 until maybe a few years ago, I don't know
12 what's -- it's said there was a sort of
13 circling of the wagons in some departments
14 and in some parts of the field, that this is
15 the way to do it and other ways are just
16 inappropriate.

17 But I don't actually sense that
18 that is a completed task. And I would urge
19 that there would continue to be a sort of a
20 hundred flowers blooming in this field.

21 I just -- it's -- Carl said it,
22 there's a notion of premature convergence.

1 Good. Let there be a variety of competing
2 explanations -- not all of them formal, by
3 the way. If the institutionalists or the
4 evolutionary people or something else can
5 explain a phenomenon better than a rational
6 actor crowd can, well, fine.

7 In fact, what I want to argue is
8 you should have contests. And I think the
9 way you would have a contest is you would --
10 you can't do what we're doing here. You
11 can't have a 2-day philosophical debate.
12 That just isn't going to cut it.

13 What you've got to do is take a
14 few substantive, concrete questions and you
15 devise a kind of a workshop setting maybe,
16 or a summer program, I'm not sure what, in
17 which you turn loose the different -- the
18 exponents of these different schools to try
19 to explain the phenomenon.

20 And you include empirical people
21 who know about the phenomenon who can say,
22 well, you know, that just doesn't comport

1 with the facts; I mean, it's a great theory
2 but you've left something critical out. It
3 could be the empiricists could simply be
4 people who are familiar with data, and maybe
5 in addition you need some people who are
6 just good methodologists -- that is, can see
7 how you would construct an appropriate test
8 of a give theory, either by regression or by
9 other means.

10 Now, what would be candidates for
11 sort of a contest like this? Well, you can
12 provide them better than I can. I mean --
13 but models of turnout in voting is the
14 obvious thing to start with, I mean,
15 something that's just totally central to the
16 field and, in my view, still hasn't been
17 resolved. There is no, to my knowledge,
18 thing that you could say in a graduate
19 course "this is the correct explanation in
20 political science for the rate of turnout
21 and why people vote."

22 I don't believe we have such a

1 theory yet. We have components of a theory,
2 there are competing points of view, there
3 are lots of coherent arguments, but it just
4 hasn't come together yet.

5 Have a contest. Culture, identity
6 and again, what I'm thinking of is you would
7 have a variety of kinds of theories and
8 approaches that would be brought in to
9 attack that kind of a problem, not just
10 formal theories.

11 Now, this brings me to another
12 point which is going to perhaps offend some
13 people. I'm sorry, but -- I love
14 experimental economics. I love behavioral
15 economics. And I'm sure that I would love
16 behavioral political science carried out in
17 the same way. But it's going to be a whole
18 lot harder in political science. And the
19 reason is that the fundamental limitations
20 to work in the laboratory are it's
21 tremendously difficult to inject the sense
22 of context, which often I believe does make

1 a difference in decision making.

2 And thus, where you can have
3 people plot-like playing kind of some sort
4 of trading game, and the context is
5 sufficiently rich so that you're probably
6 capturing the real-world phenomenon or
7 something close to it, I am very, very
8 worried that all kinds of questions in
9 political science involve so many cues,
10 psychological variables, as in escalation,
11 party platform development, ideology -- how
12 are you going to recreate plausibly these
13 kinds of motivations in a laboratory?

14 Now maybe I'm just being naive.
15 That is, maybe it is possible to do all this
16 in a convincing way. But I just -- I want
17 to raise a red flag here, or at least a
18 yellow flag, that I'm all for it, I think
19 all of these fields are better off with
20 experimentation than they ever were without
21 it-- no question about that -- and that NSF
22 should be putting money into this.

1 Still, I just want to point out
2 that I think in political science it's going
3 to be harder to draw, kind of, real-world
4 conclusions from some of those kinds of
5 experiments than it will be for some kinds
6 of similar exercises in economics.

7 MR. MCKELVEY: Let me respond to
8 that since I'm the main proponent of the
9 experimental approach. I think I agree that
10 there's no way that with laboratory
11 experiments you can address the big
12 problems. And it seems to me that the
13 question you're raising is, you know, how do
14 we build theories of big problems as opposed
15 to little problems.

16 But laboratory experiments are
17 really very good for addressing basic
18 questions about the underlying theory. So,
19 for instance, what we're looking at is
20 questions of equilibrium selection and
21 games, things of that sort, looking at very
22 simple models like the models of incomplete

1 information with juries, where they're
2 very -- you have theories which specify a
3 small number of variables which are relevant
4 and can be controlled for very precisely in
5 the laboratory. And for that kind of
6 question, I think it's -- you know, doing
7 laboratory experiments is just an ideal way
8 to go.

9 But I agree that, you know, you're
10 not going to be able to address these big
11 questions in the laboratory.

12 MR. BRADY: Well, there's also
13 field experiments as well, and I think we
14 should be willing to think about the stuff
15 that, going back to Harold F. Gosnell (?),
16 who did it in the '20s, I think it was, and
17 more recently Don Green and others. And
18 certainly in labor economics, field
19 experiments have been enormously important
20 in helping us to understand the impact of
21 labor supply and various kinds of welfare
22 policies. So we should be thinking about

1 real field experiments as well.

2 MR. SCIOLI: Jim Alt, you've
3 become a recent proponent of sharing
4 information on experiments --

5 MR. ALT: Well, no, I'm not, but I
6 think that experiments should be part of the
7 tool kit of any social scientist, and
8 political science no different from the
9 rest. And so we sponsor conferences and
10 we're pioneering a little short 4- day,
11 5-day program courtesy of Al Roth. I mean,
12 we look to behavioral economics because
13 right now they have all the money and we can
14 get them to -- you know, it's the Willie
15 Sutton principle of how to set up programs
16 in political science. So we're doing that,
17 courtesy of Becky and others.

18 My view of experiments, the
19 only -- I think the debate's a good debate.
20 I probably don't really agree with Peyton on
21 this one, largely because I think the
22 developments of the technology for

1 conducting experiments, the ability to use
2 video clips, particularly in relatively
3 natural settings, has totally revolutionized
4 the degree to which you get context across.

5 Kent Portney (?) at Tufts has
6 these wonderful sentencing simulations in
7 crime in which you just actually put it on
8 the TV and sit there in the courtroom -- you
9 know, it's not quite a courtroom, it's court
10 TV, but that's close enough -- and you just
11 watch the sentencing report be read and this
12 and that happens. And everything is the
13 same, except you never know whether the
14 defendant is going to be male or female or
15 black or white or the judge is going to be
16 male or female or black or white, and these
17 things are just dubbed in automatically as
18 needed.

19 And that gets a lot of context
20 into a very simple experiment. I mean, you
21 probably all heard -- the funniest story I
22 ever heard about this came from Shanto

1 Yengar (?) with the shopping mall
2 experiments, where they built a little
3 living room for people to sit down in so
4 that they'd believe they were watching the
5 evening news. And then they would, you
6 know, doctor the news. The problem was that
7 they had to hide the clicker because people,
8 as soon as politics came on the news, went
9 and changed the channel.

10 (Laughter)

11 But that shows -- I mean, to me,
12 you know, we laugh, but that shows how
13 realistic it must have been, you know, that
14 people really thought they were in the
15 living room and they didn't want to watch
16 the political news that night, so they
17 wanted to put on something else.

18 So what I'm feeling is these
19 innovations, these -- I don't want to say
20 you're wrong. I'd say these innovations
21 support your contention that it's difficult
22 to challenge the contention that it'll be

1 impossible.

2 MR. YOUNG: No, no, I never said
3 it would be impossible. All I said is it's
4 a lot harder. It's going to be much
5 trickier in political science to do it
6 right.

7 MR. ALT: Well, I want the
8 initiative guys who are concerned about
9 technology to notice that this is an area in
10 which the methodology is being driven by
11 gigantic technological changes that are
12 having an enormous impact on the way in
13 which a field can conduct its research.

14 MR. ACHEN: We're even putting
15 these clips onto laptops now to send out
16 into the field, so you can literally put
17 them in somebody's living room.

18 MR. ALT: Okay, and if we're going
19 that far, remember field really means field.
20 I mean, if you want to show a smiley face
21 to, you know, an Inuit or somebody in Africa
22 and all this is being done -- so, you know,

1 it opens up the world to the method in a way
2 that just never was true before.

3 MR. YOUNG: Can I get one more
4 minute of time?

5 MR. SCIOLI: Sure.

6 MR. YOUNG: I was going to pose
7 the following question just to think about
8 over the next day. In -- I think it was in
9 1901 that Hilbert proposed his 20 problems.
10 I think that's exactly what political
11 science needs to do -- needs to pose 20
12 problems, key fundamental problems. And
13 then, once you've got the problems, you can
14 bring to bear both empiricism and theory on
15 the same thing.

16 MR. SIMON: Physicists have been
17 doing that the last couple of years, exactly
18 imitating Hilbert.

19 MS. ZINNES: Let me make a quick
20 comment. First of all, about
21 experimentation, I think it's great in
22 certain domains. We tried it in IR, with

1 horrible results. Carl Getzgow (?) and the
2 inter-nation simulation was a very famous
3 attempt to put heads of states in simulated
4 environments and see what -- whether you got
5 wars and so on.

6 The problem is that it's not just
7 context. I think context is one thing, but
8 I think perhaps Jim's right. You could
9 probably get more and more of that going
10 with all the technology.

11 I think the other issue here,
12 though, is are you experimenting on the
13 thing to which you want to generalize?
14 That's the real issue. As long as you're
15 talking about people -- and people can be
16 voters, okay, or they can be -- they can be
17 a variety of things in political science
18 that have meaning in politics. But when you
19 start talking about institutions and talk
20 about nations and so on, it gets a little
21 squeamish.

22 I don't know how you do

1 experimental work on some of the major
2 problems in international politics. I'd be
3 happy to hear about it because I think it
4 would be a -- I mean, what's the value of
5 experiments? You can control them, right?
6 That's the reason you know whether something
7 affects something else, because you've got
8 the controls set -- experimentation is a
9 marvelous thing. But there are some domains
10 in political science that just don't lend
11 themselves to that unless somebody's very
12 imaginative.

13 The other thing I wanted to
14 mention is thank you for that game theory.
15 Yes, game theory is a wonderful avenue for
16 exploring all sorts of things, but again, it
17 carries with itself a set of assumptions.
18 You have to ask yourself are they relevant
19 to the question, are they germane to the
20 question you really want to investigate. In
21 some cases, yes, they're very, very
22 important, and in a lot of things in

1 American politics and maybe even comparative
2 to some extent, they are very useful ways to
3 go.

4 But in lots of things I deal with
5 in international politics, I'm not sure that
6 game theory is necessarily the way to go, or
7 even decision theory. I mean, what do you
8 have then? You have to start thinking about
9 a government as a unitary actor. Well,
10 okay, you can do that and you can get some
11 purchase on the problem. I don't know if
12 that's the most interesting way to go,
13 though.

14 So, yes, we need more than just
15 that one paradigm.

16 MR. KEECH: Well, there's -- this
17 is not to deny what you say, Dina, but
18 something is going on in which -- at
19 Carnegie Mellon in which the international
20 issues of bailouts and moral hazard and so
21 on are being made parts of experiments, and
22 they've got finance ministers and World Bank

1 officials who say they want to play these
2 games. And some of these things are
3 decisions made by just these people. So
4 there's an approach to big international
5 problems that is going to be done in a lab.

6 MS. ZINNES: Well, Carl Getzgow
7 played a lot with those things, too. He
8 took the simulations to different parts of
9 the world and different types of cultures.
10 People -- different peoples from different
11 cultures played, and he brought in State
12 Department representatives. So there's
13 always this question of -- and this is where
14 the mix between the people you're working
15 with and the context and the environment and
16 the structure within which they operate --
17 to what extent can you transfer that whole
18 thing? So, you know, this isomorphism issue
19 is really very, very important.

20 MR. SIMON: How does the
21 discussion about the usefulness of
22 experimental political science fit in with

1 the disconnect between theory and empirics?
2 I have a rough idea, but it's not clear to
3 me. Is the experimental stuff theory, is it
4 empirics? I mean --

5 MR. ALT: No, no, I mean -- sorry,
6 you go first.

7 MS. MORTON: I just -- I think
8 that there's been a real rise in
9 experimentation in political science -- not
10 just in terms of testing formal theory, but
11 also in testing social psychology and
12 psychological experiments. I mean, it's
13 just -- it's really been a big increase.
14 And I think it's because -- I think it's --
15 the formal theory part is driven by theory,
16 obviously, but I think also the social
17 psychology and the psychological increase in
18 experimentation is a desire to find -- to
19 get at a more carefully developed theory.

20 I think that's what they're
21 trying. They're not -- I'm not real pleased
22 with some of the ways they go about it, and

1 they're not really good at putting it
2 together in a coherent format that I like.
3 But I think that the rise in experimentation
4 that has occurred in political science --
5 and there's no question there's been a rise
6 in the last 30 years -- is theory-driven.
7 And I think that it plays a real big role.

8 I don't think -- I think it's a
9 complement. I don't think it's a substitute
10 for non-experimental research. But I -- so,
11 and I think that it definitely doesn't --
12 there is some context where it doesn't work
13 as well. But there's a bigger increase in
14 the psychological and the social psychology.

15 In fact, most people who do
16 experiments in political science are -- I
17 mean, probably half the formal theory people
18 who do experiments in political science are
19 sitting at this table. I mean, who else is
20 there?

21 MR. McKELVEY: Well, I mean, there
22 are a lot of economists who do --

1 MS. MORTON: No -- formal theory.

2 MR. McKELVEY: There are a lot of
3 experiments in, sort of, game theory and
4 public goods and things of this sort that
5 are done by economists which are related to
6 the questions we look at. There's, I think,
7 a very large group of people who do that.

8 MS. MORTON: They're ——. But I
9 mean just in political science.

10 MR. ALT: By the way, just to come
11 back to Carl's question, I mean, I agree
12 with exactly what Becky said. And what I
13 would add to it is it seems to me that
14 experiments are sort of part of the bridge
15 between, you know, the sort of, the more
16 micro models on which the ultimate theories
17 of legislative behavior are built.

18 And I think an appropriate
19 parallel -- it -- they substitute for data
20 we can't collect. One of the reasons that
21 people kept citing John Londregan's book on
22 Chile around the table is that this Chilean

1 constitution, for reasons that are lost to
2 me, makes you write down, you know, the
3 votes on committee decisions. And so
4 there's this mass of data that we almost
5 never get when trying to analyze the
6 positions of legislators because we don't
7 know how they behave in committees.

8 So we have theories about how
9 committees behave, and then we have
10 aggregate votes taken in the chamber, and we
11 hope and pray that our models of committee
12 decisions are right. And in John's case, he
13 was in exactly the opposite position.

14 He had this mountain of committee
15 decisions and actually no recording except
16 the ultimate winning outcomes when the thing
17 got to the chamber. Because in the chamber
18 they don't write it down, but for some
19 reason in the committee they do.

20 And so this was a
21 once-in-a-lifetime opportunity to write a
22 book actually using observational data in

1 exactly the way that I would typically think
2 we would shore up our beliefs and our models
3 by using experiments.

4 MR. STRAF: I would pose it a
5 little differently. Experiments require a
6 model that is widely accepted or widely
7 known. And I'm wondering from this
8 discussion whether a difference is that in
9 political science the models, the underlying
10 models for the experiment, may not be as
11 widely accepted.

12 Let me put this in a simple
13 framework. If you have an intervention,
14 you're either going to control for it or
15 not. If you're not going to control for it,
16 it's an observational experiment. If you
17 are going to control for it, the question
18 next is do you know the effects of the
19 secondary or confounding variables? And
20 that's what requires a model.

21 I thought Don Campbell had it
22 right. I thought his original thing with

1 quasi experiments is you control the
2 intervention but you don't know the effects
3 of the confounding variables, and that's
4 what I thought he called a quasi experiment.
5 To get to the experiment, you have to know
6 those things.

7 That's why the randomized clinical
8 trial is called the gold standard, because
9 you introduce randomization so that you know
10 the effects. Or you control the experiment
11 in a laboratory with each and every
12 secondary variable so that you know the
13 effects -- they're immaterial, they're
14 constant. The assignment to intervention in
15 both of those cases becomes independent.

16 But the crux, at least for me, is
17 that you need to understand what the effects
18 are of all these secondary and confounding
19 variables. And that's how I heard Peyton's
20 comment about context in that sense. I
21 think you're in a world where the secondary
22 variables are so subtle, so complex --

1 you're dealing with people in a highly
2 interactive environment -- and how could you
3 even begin to reproduce that, let alone
4 model that?

5 So whether you have your overall
6 theory or not, you need to be able to
7 specify a model there to have something
8 experimental. And I'm questioning whether
9 it's more difficult for social sciences, and
10 especially for political sciences as
11 compared to economics.

12 MR. ACHEN: It's really hard work.
13 I think that's the central thing that it's
14 hard to teach students. It's really, really
15 hard. There are instances in which
16 randomization is just out of the question.

17 MR. STRAF: Yes.

18 MR. ACHEN: Key problems like are
19 black people discriminated against for
20 capital punishment, you can't randomize
21 that. And we just do a multiple regression
22 and say it'll come out in the wash.

1 MR. STRAF: Without specifying
2 what the model really is, without
3 elaborating the assumptions.

4 MR. ACHEN: Doing the data
5 analysis. True.

6 MR. ALDRICH: I think this is also
7 why Becky pointed out the, sort of, social
8 psych type experimentation is advantaged
9 because it's individuals' attitudes or
10 individual decision making setting and so
11 you can cut out-- you don't have to worry
12 about the interactiveness. You can do
13 random placement assignment.

14 MS. MORTON: Yes, and, you know,
15 this -- that's where they're getting into
16 all this very, very context-rich -- you
17 know, the lab at the mall, you know, grab
18 them in, you know, sit them in the room, and
19 measuring these media effects and all that.
20 I think the -- there's a potential there to
21 connect more between, you know, these social
22 psychology experiments, but they're all

1 decision theoretic. There's no, you know --
2 so they -- at best you could say they
3 describe voters, if you think voters are
4 purely decision theoretic. But, you know,
5 they don't have any kind of strategic
6 situations or look at any kind of group
7 behavior.

8 And a lot of politics is really --
9 I think the reason why game theory is so
10 popular in political science is because so
11 much of politics is strategic, and it is the
12 only theory we have around of strategic
13 behavior, as far as I know. We have lots of
14 theories of group behavior, but it actually
15 models the strategies and the strategic-ness
16 of it.

17 And I think that's why game theory
18 is actually more useful in political science
19 than it is in economics, because in
20 economics there are many more situations
21 that are decision theoretic than there are
22 in political science.

1 So I think there's a potential to
2 tap in to, but the social psychologists are
3 doing the lab with the game theoretic
4 experiments that -- but I don't see
5 experiments as ever being a substitute for
6 real-world data. But, you know, I see them
7 as looking at smaller things, like what
8 Richard was saying.

9 Though we can do things in the lab
10 that — can't sometimes do. So --

11 MR. SCIOLI: Let me introduce Phil
12 Rubin, who joined us just a few minutes ago.
13 Phil's director of the Behavioral and
14 Cognitive Sciences Division, which is our
15 cognate division, in at one time, social
16 economic sciences, social economic
17 behavioral —. Phil is on leave as deputy
18 director of the Haskins Institute?
19 Professor of psychology and --

20 MR. RUBIN: Actually, Department
21 of Surgery now.

22 SPEAKER: How's that for

1 interdisciplinary?

2 MR. BRADBURN: Frank, before we do
3 this, could I make an observation about the
4 relationship between experiments and
5 observation? Because one of the things --
6 it's quite true, there are lots of things in
7 all the social sciences that one can't do
8 any experiments on, true experiments. But
9 then, one always can point to astronomy as
10 the quintessential observational science.

11 But the thing is, if you think
12 about why -- or one reason why astronomy
13 makes great strides is that they use
14 knowledge that has come from experiments,
15 and that has been verified in very strong
16 experiments and also very strong theory.

17 So in some sense, you get
18 everything. Even though they on the whole
19 themselves do not do experiments, they draw
20 on physics particularly -- and more and more
21 as astronomy has been interested in
22 cosmology, they're coming together -- I

1 mean, they're drawing more heavily on
2 particularly experimental physics.

3 So that there is a kind of
4 interplay between the two that they draw
5 strength from each other, and I think that's
6 something that one has to -- you can oppose
7 them, in a way, and a lot of the - - there's
8 been a tremendous amount of discussion, you
9 know, about whether experiments are the gold
10 standard and the best observations can't
11 come up to.

12 There are -- I started out life as
13 an experimentalist in social psychology, so
14 I sort of moved over to the observational
15 side because of all the difficulties that I
16 found with carrying out experiments and
17 interpreting experiments because of all
18 these contextual effects.

19 Now, the technology, as Jim
20 mentioned, has made it possible to do
21 experiments that are in some sense more
22 realistic in a way and less laboratory-

1 bound. But even so, I don't want to dampen
2 the enthusiasm in a way for experiments in
3 areas they haven't gone into before, but
4 just caution that it's not a panacea and
5 that there are lots of troubles.

6 And even natural -- I don't want
7 to say natural experiments -- the field
8 experiments, where you're trying to
9 implement a policy experimentally, or even
10 clinical trials and so forth, one of the
11 troubles when you get out of a laboratory is
12 that while you may have control over the
13 treatment, you don't have control over your
14 subjects. And they do lots of other things
15 outside the treatment. And they may get
16 treated in other ways that you don't know
17 about and they may do all sorts of other
18 things, the worst of which is to drop out.
19 And there may be not -- the drop-out from
20 real-life experiments may not be random, and
21 it may be in fact correlated with your
22 treatment or your control group or whatever.

1 So there are a lot of things about
2 interpreting realistic experiments, which we
3 think of as a wonderful thing to do, and in
4 many ways it is. But they have their own
5 set of difficulties. None of them are going
6 to be perfect, but I think -- what I would
7 stress is that we need to think not only
8 about theory and empiricism, but also
9 different types and how they play with each
10 other.

11 MR. SCIOLI: One of the things
12 that we do, for your information -- just let
13 me give you this statistic, and those of you
14 who have been on the Science Advisory Panel
15 would know, certainly Cheryl's panelists
16 know, that I'll bet we don't see two
17 proposals a year in which experimental
18 design is the methodology in political
19 science. So we don't have to worry about
20 them taking over just now with external
21 validity that's being the undermining
22 feature of the theory.

1 MR. FREEMAN: As regards the study
2 of whole systems or these large macro
3 systems that Jim and I -- ask Carl and
4 Peyton how they view computational
5 experimentation. That was something Carl
6 mentioned in his comments.

7 You know, in Minnesota, playing
8 with dynamic stochastic economies is sort of
9 where it's at. I mean, you don't estimate,
10 you calibrate. And it's the whole -- at
11 least up until recently, this was a whole
12 cottage industry and a whole generation of
13 graduate students were -- I think Lucas
14 advocates that, too, in part because of this
15 observational equivalence problem.

16 We had Axelrod (?), who I thought
17 did one of the most important works of our
18 discipline, and it hasn't really caught on
19 in political science. And that's a realm of
20 experimentation also.

21 MR. SIMON: Well, I'm not -- I
22 think it's catching on. I actually tried to

1 write about it a little bit in my handout
2 for this meeting. I put it together a
3 little bit with the complex systems approach
4 that I'm enthusiastic about, the whole
5 notion that, you know, when the theory --
6 when you -- often, to be successful, theory
7 needs to have strong assumptions.

8 When you start to remove those
9 assumptions and include things like
10 heterogeneity, organization, not perfect
11 rationality, or many actors, then you're out
12 of the realm where calculus and paper and
13 pencil will give you the answer. And to get
14 insights you need to turn to computation, I
15 think.

16 We teach it at Michigan. And as I
17 mentioned, Scott Page teaches a course on
18 computational political science at their
19 Santa Fe institute.

20 Of course, there are plenty of
21 problems. When you prove a theorem with
22 pencil and paper, you've done something very

1 general. You know what -- given some
2 assumptions, you know what follows. When
3 you do a computational run, maybe just
4 taking a few snapshots. So a theory of
5 computation needs to be developed.

6 But I'd really -- and this is a
7 case where I think political scientists in
8 the realm of -- things have been said around
9 the table, I think -- this is a realm where
10 political scientists have an advantage
11 because of the premature convergence of
12 economics on pure, simple theory and general
13 equilibrium and perfect rationality. If
14 economists are willing -- I mean, political
15 scientists are willing to develop the theory
16 with a new point of view, that could be
17 exciting. But there are dangers.

18 MR. YOUNG: Can I jump in with a
19 remark or two about this? I'm a codirector
20 of a center at Brookings that has some
21 aspects of it devoted to computational
22 modeling. This grew out of some work by

1 Joshua Epstein and Rob Axtel called
2 "Sugarscape." I won't go into the details
3 of that. But at any rate, this has evolved
4 now into a bigger platform that's similar to
5 things that go on at Santa Fe and, I'm sure,
6 Michigan and some other places.

7 The broad idea is that in some
8 computer platform you represent maybe
9 several thousand, or even it can be more
10 these days, individuals interacting in some
11 very complex landscape. And they're making
12 some kind of decision. All of that can be
13 programmed at will onto the sort of general
14 setup. And then you watch the system evolve
15 dynamically, depending on how you specify
16 the interaction structure.

17 Now, what do you learn from this?
18 Well, of course, it's not like proving a
19 theorem. You can't say that. But it might
20 tell you what theorems are provable. And
21 often actually these dynamical systems are
22 so complicated that if you even ask a good

1 mathematician what do you think is true of
2 these systems, they'll tell you -- they'll
3 conjecture incorrectly. Your intuition is
4 often very, very wrong about how these
5 systems behave.

6 So that I think that purely sort
7 of, in terms of pure knowledge, it's
8 extremely helpful to simply watch these
9 processes to see what is possible and to --
10 these surprises jump out at you.

11 Now, I personally believe that
12 Santa Fe Institute, you know, takes some of
13 this emergence a bit far. I mean — would
14 darken the room here and sort of imagine we
15 were in a shiva and wore headdresses, then
16 we could all get into the Santa Fe spirit.
17 I mean, it's almost mystical.

18 So don't take it that far. But
19 you can learn what relationships are
20 relevant, what theorems might be provable.
21 But I think there's a second thing you
22 learn. A whole lot of people who haven't

1 bought into this point of view -- that is,
2 the dynamical point of view -- you take a
3 standard-issue equilibrium economist and you
4 describe a problem. You say, well, yes,
5 suppose that people actually sort of adapt.
6 You know, they learn through some kind of
7 repeated exposure with a variety of
8 individuals and it depends who they talk to
9 and where they live and what their party
10 affiliation is. And so you describe it.

11 And they say, oh, yes, yes, I got
12 that. But then you show them,
13 computationally or projected onto a screen,
14 well, here's how it would actually look.
15 And I'm telling you, these guys say, oh, my
16 God. In other words, they hadn't really
17 understood. They had -- you know, they
18 heard the words and they sort of put it
19 together and it all sounded logical and
20 reasonable. But when you saw the dynamics
21 and the extraordinary complexity of these
22 things, it really is quite an experience,

1 and I think suggests new avenues of
2 research, new kinds of questions that you
3 can ask.

4 MR. SIMON: Let me give a -- my
5 favorite example of the last statement that
6 Peyton mentioned. I worked with a fisheries
7 ecologist, Jim Wilson of University of
8 Maine, on a complex systems version for the
9 lobster fisheries. And you can set a -- I
10 mean, the standard assumption is, in ecology
11 as in economics, is that everything is
12 uniformly distributed -- the fish are
13 uniformly distributed in the water, they're
14 all the same size, the fishermen are
15 uniformly distributed in the water. And
16 when you take that assumption away, you've
17 introduced complexities that you really do
18 need to use a computer to understand.

19 But the lobster fishermen in Maine
20 have a stricter conservation law than the
21 fishermen in nearby states and in Canada;
22 namely, you can only catch lobsters of a

1 certain size -- 1 to 2 pounds. That's
2 universal, but in Maine, if you caught one
3 that was either pregnant or the wrong size,
4 you had to notch the tail so that no one
5 else could -- that lobster had made it. No
6 one else could catch it. And they did --
7 they had to release it right away.

8 Well, the New Hampshire fishermen
9 didn't have this rule. And of course
10 lobsters migrate, and they were just, you
11 know, waiting at the border for notched
12 lobsters to come on, salivating. And the
13 Maine fishermen said, well, you know, here's
14 a common problem. Why the hell should we
15 have this self-imposed discipline when our
16 neighbors are taking advantage of it?

17 So Jim Wilson took much of the
18 work that we had talked about, but he was
19 able to put it on a computer to do a
20 simulation that really captured the essence
21 of what was happening. So much so, he could
22 take it to the town meetings among the Maine

1 fishermen and convince them that it was
2 realistic, so they could see what was
3 happening, and then ask them to play with it
4 and, particularly, see what would happen if
5 they changed their behavior. And he used
6 this method to convince them to not change
7 their behavior. Eventually they were strong
8 enough to change the behavior of the
9 neighboring states, so it's a victory for
10 the good guys and the lobsters.

11 But, you know, an indication how
12 seeing something in a simulation developed
13 can really add a lot of insight. Whether
14 it's theory or not is not quite clear, but
15 it's not so far.

16 MS. ZINNES: Let me make a comment
17 on that because my colleague mathematician
18 Bob Lancaster (?) and I had a very similar
19 experience. We had been playing with a
20 model, differential equations for modeling
21 the friend of my friend, you know, this
22 little adage -- the friend of my friend is

1 my friend, but the enemy of my friend is my
2 enemy, et cetera. And we set this up as a
3 series of differential equations and were
4 interested in seeing what would happen over
5 time, particularly since people like Heider
6 (?) had argued that, you know, you move
7 towards balance, right, and in the graph
8 theoretic context that he developed his
9 models.

10 And what we discovered in putting
11 these into a computational model, putting
12 this into a simulation setting, was that
13 even if -- whether you started with three
14 nations or five nations or whatever, you're
15 less polarized; i.e., you always got a
16 balanced system in good old graph theory
17 terms.

18 And this was so interesting -- it
19 didn't matter how you started out; that is,
20 if you started off by initializing with
21 respect to which countries are friends and
22 which ones are enemies and so on and then

1 you -- the equations kind of tell you how
2 that moves forward. And the interesting
3 this is that it always polarizes in the
4 sense that either you get two opposing
5 parties or you get everybody together as one
6 group. You never get a tripolar system, et
7 cetera.

8 Now, this was very, very
9 interesting to one of Bob's students in
10 mathematics, said how can that be, and he
11 went and proved, actually, a theorem that
12 showed -- it's very specialized, okay, our
13 simulations are much, much more general.
14 But he was able to prove under certain
15 conditions with a three- nation system and
16 certain restrictions why that comes about.
17 So the synergism between the use of
18 computational modeling and proving theorems
19 is just very important.

20 But I wanted to just make one
21 other comment about simulations. The thing
22 that has disturbed me about the simulations

1 that go on in political science is there
2 tends to be a lack of understanding of what
3 the output is. That is, people call that
4 stuff data. And I don't see it as data. It
5 seems to me, when you set up your simulation
6 you have programmed in, essentially, your
7 assumptions and your theory.

8 And what you're doing with that
9 simulation is you're finding out, given
10 those assumptions, what kinds of things
11 happen as a consequence. You're not
12 generating data. Now, you might want to
13 call it data, but you'd better call it
14 theoretical data or something to distinguish
15 it, because it's not -- I don't know if
16 you'd -- the term "observational." It's not
17 real-world kind of data.

18 And there has gotten to be kind of
19 a misunderstanding here that simulation is
20 like an experiment, that you are really
21 generating observational data. That's not
22 true. You really are generating deductions

1 from your theoretical structure. It's a
2 great way to find out things about the
3 analytics, but I don't think it tells you an
4 awful lot about how people behave. That's
5 another step.

6 MR. ACHEN: I've seen people run
7 regressions on it, actually.

8 MS. ZINNES: I know — are
9 further deductions -- this goes with this
10 under that set of circumstances. But I --

11 MR. SIMON: Which is fine, but --

12 MS. ZINNES: Yes, which is fine,
13 absolutely fine.

14 MR. SIMON: I'm mean, in the
15 Sugarscape model there was some question
16 about how does inheritance affect the way
17 people act and that why they're stifling --
18 what is it -- the former vice presidential
19 candidate from Indiana --

20 SPEAKER: Quayle.

21 MR. SIMON: The Quayle effect,
22 where in fact inheritance makes the system

1 go downhill rather dramatically.

2 Anyway, but -- you know, it's an
3 experiment run within -- by changing some
4 parameters you can ask how will this affect
5 things in a situation where you couldn't in
6 the -- in epidemiology, some of these
7 computer experiments are crucial because you
8 couldn't do the experiments in the real --
9 it would be unethical to do some of the
10 experiments. And they give insights that
11 epidemiologists there cherish.

12 MR.GRANATO: For the record,
13 "Sugarscape" is in the book, Growing
14 Artificial Societies, for those of you who
15 haven't read it. And it's by Joshua Epstein
16 and Robert Axtel. And it's fascinating
17 stuff.

18 What we're going to do now is
19 we're going to collapse discussion point 2
20 and discussion point 4 to later on today.
21 What we're going to do after a break is have
22 Cheryl take the lead on the

1 interdisciplinary initiatives here and what
2 she's seeing in her program.

3 So let's come back at about 3:00.

4 (Recess)

5 MR.GRANATO: We're ready to start
6 discussion point 3. Cheryl.

7 MS. EAVEY: I guess that's me.
8 I'm the interdisciplinary person, I'm no
9 longer a political scientist -- which I
10 think is probably true, having run MMS for
11 the last 8 years. It certainly gives one a
12 different perspective on things. And I
13 assume I'm talking because there's a
14 potential for the linkages that you're
15 trying to gain a handle on to be enhanced by
16 thinking beyond political science. And we
17 do that kind of naturally with economics and
18 the field of political economy, but the
19 question is are there advantages to going
20 beyond that.

21 And I guess in general I think
22 there are advantages, as simple as bringing

1 in different kinds of expertise. So if
2 you're a political scientist you may want to
3 pair with a statistician to create a
4 statistical model, or a mathematician to
5 create a mathematical model.

6 Going beyond your discipline
7 lowers your comfort level in some sense, and
8 that forces you to challenge the assumptions
9 that you're making, and I think there are
10 advantages there.

11 So the notion of pairing with
12 other individuals and merging multiple
13 approaches and multiple techniques is one
14 that generally NSF has been encouraging and
15 one that I guess I find appealing, and one
16 that perhaps would be interesting to see how
17 you collectively feel about it with respect
18 to the issues that we're discussing today.

19 I'm not going to talk about the
20 academic examples and the institute examples
21 that Jim and Frank listed on the agenda,
22 because there are people here from those

1 institutions who know them much more
2 intimately than I do and can speak to them.
3 What I thought I would do is just give some
4 examples, both of projects and mechanisms,
5 that have come across my desk in some sense
6 ways of facilitating these interdisciplinary
7 collaborations.

8 The first is a training example,
9 and that's not even really a collaboration
10 per se. As some of you may or may not know,
11 the MMS program has something called
12 mid-career opportunity awards. And what we
13 do with mid-career is we essentially buy off
14 someone's time.

15 So we take a linguist and we put
16 them in a math department -- which we've
17 done -- so they can create mathematical
18 models of linguistic phenomena. We'll take
19 a political scientist and put them in a
20 statistics department, or a statistician and
21 put them in a sociology department. And
22 it's a way of gaining skills that they

1 didn't have before and potentially
2 establishing collaborations with members of
3 those departments.

4 The most common way, perhaps, that
5 we facilitate interdisciplinary work is
6 through collaboration, supporting
7 collaborative work. And we do that at a
8 small scale and at a large scale, and we do
9 it at various points throughout the
10 foundation.

11 Both Frank's program, my program,
12 and other programs within the divisions can
13 support small collaborations; i.e., two or
14 three individuals from different disciplines
15 working together.

16 And these can be very fruitful.
17 One of the collaborations that comes to my
18 mind is Kim Romney and Bill Batcholder (?).
19 Romney is an anthropologist, Bill is a
20 psychologist. Together they created
21 something called cultural consensus theory
22 which you might be interested in -- which is

1 a measurement technique for culture, formal
2 technique. They've also done empirical
3 testing of it. Their work blends
4 computational, statistical, and mathematical
5 aspects. So it's highly rigorous and a very
6 fruitful collaboration.

7 We've funded psychologists and
8 statisticians working together to create
9 statistical models of developmental
10 processes. So there are lots of
11 collaborations writ small that programs can
12 support within program budgets.

13 There are other types of
14 activities that we can support to further
15 these interdisciplinary processes. And I'll
16 go back to the Manski example I talked about
17 earlier. We can obviously support workshops
18 like this workshop, although you're not very
19 interdisciplinary and I think your comfort
20 level is probably too high.

21 But we can support activities, you
22 know, where you're much more

1 interdisciplinary and your comfort level is
2 much lower and you'll get very different
3 kinds of discussions. Those tend to be
4 one-shot deals. Those may not be the most
5 fruitful avenues.

6 But what we're exploring a little
7 bit, and my program is not the only one
8 doing this, is seeing if we can emulate,
9 writ small, what MacArthur has done very
10 successfully in terms of creating these
11 networks of researchers. Indeed, it's the
12 MacArthur Foundation that I think is taking
13 experimentalists like Jean Ensinger (?) and
14 putting her in the field with a laptop
15 computer.

16 What I'm doing with Chuck Manski
17 is we're creating a network of individuals
18 interested in social interactions from
19 across disciplines, from different
20 perspectives -- theoretical, empirical -- to
21 engage in conversations over time. Because,
22 as Chuck has claimed, theory is way ahead of

1 the empirical work and the two groups don't
2 talk to each other.

3 So that's another mechanism,
4 another way that NSF can further these
5 interdisciplinary activities.

6 We also, although usually not
7 through our regular programs like Political
8 Science or MMS, can support much larger
9 collaborations. And these can really be
10 fascinating activities with lots of synergy.

11 One good example, I think, for
12 political science and for other fields would
13 be the biocomplexity competition, which has
14 been running for the last 2 or 3 years.
15 These are large-scale projects where we're
16 looking at modeling, a lot of agent-based
17 type modeling or other types of modeling,
18 human natural interactions, and we're making
19 awards to ecologists working with
20 economists, working with anthropologists,
21 working with mathematicians, all on the
22 modeling of various coupled human natural

1 systems. And those --

2 MR. BRADBURN: A political
3 scientist is the head of one of them.

4 MS. EAVEY: I know, but I was
5 getting to that as part of my next example.
6 That's true, a political scientist is the
7 head of one of them.

8 And those sorts of collaborations
9 we can fund at a higher, longer level than
10 you would get with regular NSF programs, so
11 you can talk about maybe \$250,000 - \$400,000
12 a year for a period of 5 years, something
13 like that. Biocomplexity is an ongoing
14 competition. If you have an interest in the
15 natural environment, human natural
16 environment, it may be a very good avenue to
17 pursue.

18 Very occasionally we have funds
19 that allow us to produce or to support very
20 large-scale interdisciplinary collaborations
21 over long periods of time, usually with
22 other centers. And out of those types of

1 activities, I think, comes some very
2 exciting work. The best example I have that
3 includes a political scientist is CSIPEC --
4 Center for the Studies of Institutions,
5 Population, and Environmental Change -- at
6 Indiana University.

7 And I don't know how broadly the
8 political science profession knows what Lynn
9 Ostrom (?) does in her spare time, when
10 she's not being sort of a pure political
11 scientist, but Lynn is one of the
12 co-directors of CSIPEC, along with Emilio
13 Moran (?), who's an anthropologist. They
14 have an award, 8-year center award, that
15 brings together ecologists, biologists,
16 anthropologists, obviously political
17 scientists, economists, formal modelers, all
18 looking at deforestation and afforestation
19 in the Americas.

20 And the CSIPEC approach is truly
21 unique, because here really is a pairing of
22 the modeling with the empirical. They are

1 blending multiple approaches from different
2 disciplines, from ecology, from political
3 science, collective action kind of pool of
4 resources -- Lynn's work in that area --
5 with going out into the field and collecting
6 data on soil samples, household data, census
7 survey type household data; taking that
8 data, merging it with GIS, remote sensing,
9 data at lots of different levels on
10 different scales, different theoretical
11 approaches, to try to lend these to bring
12 insight into the study of afforestation and
13 deforestation.

14 And it is a very, very interesting
15 approach and one that does blend, again,
16 theory with the empirical. And it's also
17 interesting from kind of a philosophy of
18 science perspective, because if you go to
19 CSIPEC you can talk to any of the graduate
20 students there and they can tell you a
21 little bit about GIS and a little bit about
22 remote sensing and a little bit about every

1 aspect of the project even if they're an
2 economist or a political scientist. Because
3 they all believe that they all have to know
4 a little bit about everything. So it's also
5 a wonderful training ground.

6 So there's lots of different
7 avenues at NSF for interdisciplinary work to
8 be supported and sometimes at very
9 substantial levels. And this blending of
10 different kinds of expertise may be one way
11 to forge a tighter link between theory and
12 empirical work.

13 And that's all I have to say.

14 MS. MORTON: What was the
15 political science example that these guys
16 alluded to?

17 MS. EAVEY: Oh, that was CSIPEC.

18 MS. MORTON: Oh, there wasn't some
19 other one?

20 MR. SIMON: I thought you said
21 Lynn Ostrom biocomplexity grant that --

22 MS. EAVEY: Oh. Well, in addition

1 to the funding for the center, they also won
2 a biocomplexity award. Oh, that's actually
3 a good point, because now, with everything
4 else that they're doing, they've laid on
5 agent-based modeling. And they've brought
6 in Jerry Busmeyer (?), who's a psychologist,
7 to take the lead on the modeling aspect.
8 And this is going to be part of their
9 overall framework for the center.

10 MS. MORTON: I had another
11 question. When you talk about these guys
12 getting together, like the anthropologist
13 and the --

14 MS. EAVEY: And the psychologist?

15 MS. MORTON: Yes. They got
16 together first and then applied to you,
17 right? I mean, do you -- is there a way
18 that you, like, draw those guy and put them
19 in touch with each other?

20 MS. EAVEY: Well, that's a very
21 interesting question. Yes and no. A lot of
22 times people will come to us with

1 already-formed collaborations. So, for
2 example, the program has supported Bernie
3 Grafman (?) working with Marley (?) and
4 Regenbether (?), two psychologists. And
5 that collaboration came in to us as is.

6 The program also supports another
7 psychologist and statistician working
8 together. We actually created that
9 collaboration. We had a psychologist come
10 in by himself and basically told him it
11 looks interesting, find a statistician and
12 come back. And he did. And it's been a
13 very fruitful collaboration.

14 With the biocomplexity
15 competition, one of the unique feature of
16 that competition has been what we call
17 incubation awards. And incubation awards
18 are in some sense trying to get at what
19 you're talking about. That is, we've
20 reserved -- I think this year it's a million
21 dollars, which has been set aside to fund
22 small proposals that are incubators -- are a

1 series of workshops, kind of like a planning
2 grant. Because we recognize that many of
3 these collaborations that we would like to
4 support with larger awards need time to gel.
5 And so the incubation award is to give these
6 folks time to, in some sense, get together
7 and figure out what they're doing and learn
8 each other's language.

9 Now, it's interesting with CSIPEC.
10 With CSIPEC the folks there knew each other
11 but had never really worked together. And
12 Lynn and Emilio say it took about 2 years,
13 or a year and a half, for them to have a
14 common language in terms of what they were
15 doing.

16 MR. RUBIN: Yes, let me give you
17 an example. Carl and I just spent the
18 weekend at a workshop at the American
19 Academy of Arts and Sciences in Cambridge,
20 and it was the result of an incubation
21 award. And there were -- it was geared
22 towards eventually developing perhaps a

1 proposal, a large-scale proposal for the
2 biocomplexity of the environment.

3 At that meeting were geneticists,
4 anthropologists, psychologists, people in
5 computational modeling, on and on and on.
6 It was a pretty wide group. And there was a
7 total of 40 people. It's going to be
8 followed up by a more focused group,
9 perhaps, of about five to six people who on
10 their own intend to develop then a
11 full-scale proposal for the biocomplexity
12 competition.

13 MR. SIMON: I was at one the
14 previous week, too, and that was one about
15 modeling human- animal interactions, animal
16 conservation. And again, it was a pretty
17 interesting mixed group, a lot of people who
18 work in the field, especially Africa,
19 working on animal problems -- tuberculosis
20 in buffalo or just deer getting in the way
21 in suburbs. But it was, again, economists
22 -- no political scientists -- a lot of

1 biologists, one math modeler, and -- you
2 know, a lot of energy in these meetings.

3 MS. EAVEY: There are, yes. It
4 can be very exciting stuff.

5 MR. YOUNG: How do you construct
6 the panels that review the proposals for
7 things like this?

8 MS. EAVEY: Oh, very carefully.
9 You know, it's always a challenge. What we
10 try to do, what you try to do generally when
11 you're looking -- when you've got a group of
12 proposals that are interdisciplinary in
13 nature. You're obviously bringing people in
14 from different disciplines, right, who have
15 disciplinary perspectives and whatever
16 biases go along with those. You try to have
17 individuals, however, who have breadth and
18 appreciation for work that goes beyond their
19 discipline or perhaps beyond their
20 particular area of expertise.

21 And if you identify good people
22 with breadth and if you try to carefully

1 look at the proposals at hand to the extent
2 that you can and make sure that all areas
3 are covered -- sometimes that means having
4 outside reviews from disciplinary experts in
5 addition to a panel -- you can get what I
6 think are very, very reasonable methods for
7 evaluating these proposals. It's not a
8 problem; it just takes care.

9 MR. KEECH: How is this related to
10 IGERT? Is that graduate training oriented,
11 — and research?

12 MS. EAVEY: IGERT is graduate --
13 that's correct. And that's, you know,
14 perhaps one way that MMS and Political
15 Science will fail you guys, is that we're
16 not well equipped to -- our programs are not
17 well equipped to fund education. We've done
18 teeny bits -- the MMS mid-career, both
19 Political Science and Economics supported
20 Dina's workshop master's activities for a
21 short time. But generally speaking, we're a
22 research directorate so our ability to

1 support education tends to be somewhat
2 limited.

3 IGERT is out of the Education
4 department. It is graduate education and
5 training, and is potentially a good
6 mechanism for some of the things that have
7 been discussed today.

8 MR. BRADBURN: Not the Education
9 department, the Education directorate.

10 MS. EAVEY: I'm sorry. I've only
11 been here 8 years, I should know this.

12 MS. MORTON: What about post-docs?
13 Is that --

14 MS. EAVEY: Post-docs can be
15 supported on part of a regular award.

16 MS. MORTON: Okay.

17 MR. BRADBURN: Let me just say
18 that as part of the -- if we're successful
19 in getting extra funds in this priority
20 area, one of the things we'd like to do is
21 to institute some IGERT-type programs within
22 the social sciences. Right now one of the

1 difficulties -- I think some of you have
2 applied, or at least the ones I've talked to
3 -- is that the — by and large is defined
4 as something in two directorates of NSF. So
5 that's not entirely true. There's one at
6 Harvard on poverty that's just social
7 sciences. But mostly the IGERT, sort of,
8 ruling is the way you establish
9 multidisciplinary is that it's as
10 disciplines are defined in NSF by the
11 directorates. So it's been hard for social
12 scientists to be -- I mean, like economists
13 and psychologists working together, that's
14 in some sense a broad span.

15 MS. EAVEY: Yes, but we could do
16 political scientists and mathematicians and
17 statisticians working together.

18 MR. BRADBURN: That's true.

19 MR. SIMON: We just put in an
20 IGERT for, sort of, using computational
21 point of view to study political and
22 economic institutions. I guess what you're

1 saying is because it's too social science
2 oriented, it's chances are weaker.

3 MR. BRADBURN: Well, if it's -- if
4 the computational part attracts either -- it
5 would be in SEIS (?) or the math and
6 statistics group, then that would be fine.

7 MS. EAVEY: There is another
8 priority area on the horizon which could be
9 of interest, and it didn't fare well in the
10 last budget, but that's the math initiative.
11 And if the math initiative is ever more
12 fully funded, there will be an
13 interdisciplinary component to -- I'm sorry,
14 priority area, not initiative -- there will
15 be an interdisciplinary component to that
16 priority area. And the general idea we have
17 now is for, let's say, SBE to pair with
18 statisticians or mathematicians and to hold
19 a competition for mathematical research in
20 social behavioral science areas.

21 So that is a competition which
22 could greatly benefit our disciplines and

1 perhaps be relevant for what you're
2 discussing here, assuming that we can
3 actually get it funded.

4 MR. RUBIN: Three other crossing
5 kind of programs, actually four, are we have
6 an infrastructure competition and we've
7 given a series of awards over the past few
8 years that provide for larger-scale funding
9 and can be both -- we like to see them
10 crossing the disciplines within SBE, but
11 some of them often are not as broad as we
12 like. But they tend to pioneer work in
13 different things like digital libraries or
14 new sources of data, and Norman can talk
15 more about those if he hasn't already.

16 We have the MRI, major research
17 instrumentation. I just saw the awards, and
18 I know that there weren't any competitors
19 even in political science. There were
20 awards made mostly in GRS -- that's
21 geography regional sciences -- human
22 cognition; there was one in sociology that

1 was not made, but there was one competitor.
2 So the community needs to be aware of
3 whether or not there are major
4 instrumentation needs.

5 And if it hasn't been mentioned,
6 the ITR competition -- that's information
7 technology; and finally, the new advance
8 program, which is the replacement for their
9 power (?) program -- are some of the other
10 opportunities. There are many, many
11 different things. I don't want to --

12 MS. EAVEY: Oh, that's fine. You
13 need to be aware, though, because they come
14 and go. So for example, we had
15 infrastructure for 2 years; we're not
16 planning a competition for fiscal year 2002.
17 So you need to pay attention, because these
18 things do come and go, and you need to catch
19 it while it's available.

20 MR. SCIOLI: Hopefully with the
21 priority initiative or priority activity
22 these kinds of activities will be much more

1 stable in terms of the funding opportunities
2 and the consistency or the persistency of
3 the program. The ITR, which Philip
4 mentioned, and which you should take a peek
5 at on your Web site, this group in
6 particular would find it fascinating,
7 interesting in terms of the opportunities
8 for information and technology broadly
9 defined.

10 And what we do, in response to I
11 believe it was Peyton's question about how
12 do we constitute the panels, within programs
13 we constitute the carefully, we hope, but
14 with regard to these initiatives, I mean we
15 get tremendous urging from Norman and from
16 Philip and Bill to make sure that we're at
17 the table when these proposals are evaluated
18 not us, but our disciplines.

19 So if we have submissions that are
20 multidisciplinary and involved statistics,
21 political science, sociology, we try to get
22 as many representatives as possible to

1 advise the foundation on these activities.

2 So when -- you know, it's the
3 small- numbers problem or the no-numbers
4 problem, as Philip mentioned with MRI, that
5 it becomes a problem, but with ITR, I
6 attended a meeting 2 weeks ago, largely
7 where engineers were told about engineering
8 infrastructure and the technology as it
9 affects engineering infrastructure. And
10 when I was invited by Priscilla Nelson to
11 talk, and I said to folks, hey, if you know
12 sociologists, psychologists, methodologists,
13 political scientists, please involve them in
14 these projects. We really encourage that.

15 And, you know, the engineers where
16 just, what, you mean we can bring other
17 people? Of course, it considerably affects
18 the chances that something like that will be
19 evaluated favorably, because it has a
20 synergistic effect. We see proposals, we
21 try to get more people on the panels, the
22 people on the panels are encouraged to look

1 at these issues broadly writ.

2 And so that's the
3 multidisciplinary research activities. And
4 Cheryl's correctly highlighted the ones
5 where this workshop appropriately relates to
6 the Ostrom work and some of the other. But
7 they're few and far between, wouldn't you
8 agree, Cheryl?

9 MS. EAVEY: Absolutely.

10 MR.GRANATO: Does anybody have
11 anything else?

12 MR. SCIOLI: How about something,
13 if we would -- Cheryl referred to Carnegie
14 Mellon, Cal Tech, Brookings, and maybe you
15 can say something about Santa Fe as well.
16 But can you share with us the -- for the
17 record. Certainly we all know about it, but
18 we have visitors here who might be informed
19 about what's going on.

20 MR. KEECH: Well, let me say
21 something about Carnegie Mellon. Richard
22 has already indicated that at Cal Tech some

1 of these things are just not problems. And
2 I'm pleased to say that that is the case at
3 Carnegie Mellon as well. I know my own
4 department, Social and Decision Sciences,
5 best.

6 Actually, I'm aware of Cal Tech,
7 having spent 6 months there about 10 years
8 ago, and Cal Tech was my model for what
9 Carnegie Mellon ought to be as an
10 interdisciplinary place.

11 My department is actually more
12 diverse, I think, than Cal Tech was, Cal
13 Tech being pretty purely economics and
14 political science. We have sociologists,
15 psychologists, historian, industrial
16 engineer, philosopher all in a pretty
17 coherent program, but that is considerably
18 more diversity.

19 There's just no sense of hostility
20 to modeling or formal theory. In a sense
21 this ironic because we are a citadel of
22 bounded rationality and of critiques of

1 rational choice modeling. But we critique
2 them from an informed and sympathetic point
3 of view rather than from know-nothing
4 hostility against anything formal, which is
5 something I've seen a lot of in political
6 science.

7 So up until recently we hadn't
8 been all that theoretical, actually. We
9 hired John Pali (?) from Cal Tech and we
10 have another -- well, actually, we're sort
11 of a Cal Tech -- pretty good for you guys.
12 Three Cal Tech people in recent years in two
13 units at Carnegie Mellon. But we hire them
14 because they are compatible with us. But
15 another thing that's happened at Cal Tech, I
16 think, is that it's become a little broader
17 and more behavioral with Colin Camber (?)
18 among perhaps others.

19 So I don't know how you would
20 create this atmosphere. I came from a place
21 where I would love to have created it, and
22 its absence was one of the reasons I wanted

1 to go to a place like Carnegie Mellon. But
2 I guess one thing to -- I'm trying to think
3 what could be imitated, and I may need to
4 think about that a little bit longer. But
5 there's that word again, the culture of the
6 place is very interdisciplinary and
7 disciplinary boundaries are low, if they
8 exist at all. They're not a political
9 science department. We've made substantial
10 contributions to political science, first in
11 the old SUPA, School of Urban and Public
12 Affairs, which became the Heinz School.
13 Richard was there, along with Peter
14 Ortterschuk and Mel Hennick.

15 And there was another phase of
16 contribution to political economy coming out
17 of GSIA, the business school, under the
18 leadership of Alan Meltzer and with Keith
19 Poole and Howard Rosenthal and Tom Rohmer,
20 et cetera, Alberto Alazina.

21 So now political science is mainly
22 the job of my department. And we're small,

1 but we are analytical and we are doing the
2 kind of undergraduate training that -- to a
3 not as big an audience as I would like, but
4 of the kind that I read in some of the memos
5 and heard in some of the comments made this
6 morning.

7 And I must say that the audience
8 is a little different as well. I think one
9 of the things that makes political science
10 undergraduate training less theoretical as
11 something that's not totally under our
12 control, it's the expectations of people who
13 come to college. And I don't know where
14 they get it, but they expect economics to be
15 analytical and they expect us not to be.

16 In the Carnegie Mellon they don't
17 suffer from this expectation, or they don't
18 come expecting political science much at
19 all, but students are not thrown by an
20 equation or a model and, you know, there are
21 drama students and all kinds of students
22 there, but it's just a very -- from the

1 point of view of what's shared in this room
2 a very fortunate and ——.

3 MR. McKELVEY: The unique thing
4 about Cal Tech is the interdisciplinary
5 nature, where it brings together economists
6 and political scientists primarily. We also
7 have some historians, an anthropologist, and
8 sort of a behavioral psychologist, Colin
9 Camber, who does sort of psychology, really,
10 but economics research.

11 I guess one of the things about
12 Cal Tech is that it's first to be small.
13 And because of the fact that we support both
14 the economics and political science
15 departments within this one division --
16 actually it's half of the division if you
17 —— in social sciences -- we don't have a
18 lot of fat. And since we can't afford to
19 build up an entire political science
20 department or an entire economics
21 department, and so we're sort of forced
22 towards the center. And consequently we

1 have a program which is very centered around
2 common elements between economics and
3 political science, as we have selected them,
4 namely the primarily quantitative aspects of
5 their connection.

6 So there's a lot of -- well, the
7 graduate program is focused around the
8 quantitative aspects and the -- we don't
9 have a political science degree, we have a
10 degree -- at the PhD level, we just have a
11 social science degree.

12 So someone comes into our program
13 and both the economists and the political
14 sciences take the same courses through the
15 first year and then they start to
16 specialize.

17 Even, I guess -- but I don't see a
18 lot of the problems that we've been talking
19 about here. That's not to say that -- I
20 don't know, I guess I exaggerate a little
21 bit, but even in a department our size there
22 is mostly an applied-theory tension always

1 when you decide you're going to hire someone
2 new, you know, which direction you're going
3 to go. But there's certainly, I think -- we
4 tend to respect the empirical people, they
5 respect us, and there's a lot of interaction
6 between us. We go to common seminars and so
7 on.

8 As far as Cal Tech providing the
9 model for other places, I think -- I mean,
10 Cal Tech and Carnegie Mellon start off with
11 a different view of how the university is
12 organized than a lot of other universities
13 do. And, you know, I think it's -- you
14 know, it works really well in a place like
15 Cal Tech. I'm not sure to what extent you
16 could take this model and import it to other
17 universities. Part of the way we go there
18 is through just the, sort of, historical way
19 in which Cal Tech is organized. So I don't
20 know the degree to which you can export
21 this.

22 But I think as far as the

1 training, the students that we produce, I
2 think they probably end up a little sort of
3 heavy on the theoretical end of the
4 spectrum. And one of the problems when they
5 go out to get jobs is that they -- I think
6 that they're frequently seen as not having,
7 you know, enough empirical background. So
8 we've recently actually gone from a 4-year
9 program to a 5-year program at our PhD level
10 to try and help to alleviate this
11 difficulty.

12 At the undergraduate level I think
13 we have sort of a similar experience to
14 Carnegie Mellon. The students at Cal Tech
15 are -- you know, they don't come there for
16 social sciences, they come for the natural
17 sciences and they are very quantitatively
18 tooled up by the time -- you know, when they
19 get there. The median score on the analytic
20 SATs is 800. So we don't have anything to
21 worry about with them being prepared for the
22 analytic part of the courses that we teach.

1 Consequently the undergraduate
2 courses that we teach, the introductory
3 political science course is taught out of
4 Peter Ortterschuk's book, you know, so it's
5 not what you would get at a standard
6 introductory level political science course.
7 It is sort of focused towards a lot of the
8 theoretical questions that people here are
9 interested in.

10 I don't know how effective that is
11 in terms of producing the students that
12 eventually will go on into this field
13 because I think a lot of the students have
14 already decided what they want to go into
15 and they just take the social science
16 because it's a requirement.

17 But I don't know. I think maybe
18 I'll stop here.

19 MS. MORTON: One thing I wanted --
20 about interdisciplinary stuff that -- it
21 seems to me that even when you're a normal
22 university which has normal departments, and

1 you know, it's incredibly hard for junior
2 people to try to be interdisciplinary.
3 Because they just -- you know, they don't
4 end up getting tenure if they try to do
5 this. I mean, you have to publish in your
6 discipline's journals or books in your
7 discipline. So the only people who can
8 afford to do these sort of things have to be
9 the more senior people.

10 So it's not clear to me that this
11 is a way to help build up junior people with
12 these skills, because the people that can
13 actually -- you know, by the time you are
14 able to take advantage of some of these
15 initiatives, you're maybe past the point
16 where we want to reach you.

17 I mean, I -- and so I'm not
18 convinced that this is the way to go unless
19 we can figure out this way to help -- I mean
20 the junior people in departments, like at
21 Iowa, if they go and spend a whole lot of
22 time and try to publish in statistics

1 journals, it's going to be a problem for
2 them. I mean, Iowa actually may be more
3 friendly to this than some other places, but
4 most departments are going to be suspicious
5 of things you do with other disciplines.
6 And it's also a big investment in time.

7 I'm really very friendly toward
8 interdisciplinary work, but I don't see that
9 this -- the problem is this institutional
10 structure that we have to live in, and I
11 don't know how -- I think NSF is doing
12 everything they can, but they can't go and
13 make the deans happier with this. And I
14 don't know what to do about that.

15 MR. YOUNG: Can I cut in and just
16 make a comment about that? I certainly
17 agree that the incentives for graduate
18 students are like that, but actually
19 undergraduates I think is a different story.
20 And if one can inject in the curricula
21 things that are frankly interdisciplinary, I
22 think you'll turn on smart undergraduates.

1 They don't have to worry about -- you know,
2 they're not on the track that you're talking
3 about. They're just exploring their lives,
4 and often I think feel very constrained by
5 the standard things that we teach in
6 economics or political science or whatever
7 the heck it is.

8 So I find at Hopkins -- I was
9 going to talk about in a minute -- we've
10 offered some interdisciplinary courses based
11 on, say, computational modeling or the
12 social sciences, and they're breaking down
13 the doors to get into these courses.

14 MR. BRADY: I think, too, that
15 things have changed with economics and
16 political science in the last 20 years.
17 When I was doing my PhD work at MIT, I
18 remember, I wandered between economics and
19 political science and it was like two
20 absolutely different worlds. They couldn't
21 understand why -- each side could not
22 understand why I was taking courses in the

1 other place.

2 But now I see, at Berkeley
3 especially, at least two really important
4 points of contact. One is the behavioral
5 economics stuff, which is just a great point
6 of contact for us in political science; and
7 the other one is this increasing interest on
8 the part of economists in macro issues
9 regarding transitions. And that's a
10 tremendous point of contact with political
11 scientists, because they have a sense they
12 need to know more about that. And if you
13 can get the right kind of political
14 scientist, like a Jim Robinson who we have
15 at Berkeley, that can just be a tremendous
16 way to have some relationship.

17 What we're trying to think about
18 now is actually establishing a joint program
19 with economics that would be called Politics
20 and Models -- and maybe economics in there
21 as well -- that would try to put all those
22 things together into something that students

1 could do. I think products of such a
2 program like that would be very employable.
3 Maybe others worry that they wouldn't be,
4 but I would think that would be a tremendous
5 kind of thing to get going. And it's not
6 quite the Cal Tech model, but it's one way
7 to do it.

8 MS. MORTON: But that wouldn't
9 qualify. That's not interdisciplinary
10 enough. Right?

11 MR. BRADY: What I'm talking
12 about?

13 MS. MORTON: No, I'm talking to
14 Cheryl. Politics and economics, they're
15 not --

16 MS. EAVEY: It's certainly
17 interdisciplinary. It has a long tradition.
18 If you want to know what CSIPEC has done, if
19 I can remember correctly, CSIPEC -- did I
20 interrupt you, Henry?

21 MR. BRADY: No, no, no.

22 MS. EAVEY: CSIPEC had something

1 like five post-docs that they filled one a
2 year. And with those -- in addition to
3 those post-docs, the institution made a
4 commitment to create -- most post-docs are
5 funded by NSF, but the institution made a
6 commitment to create five tenure-track lines
7 that would reside in the departments, that
8 the committee that would choose which
9 department would receive the line would be a
10 multidisciplinary committee with CSIPEC
11 members and essentially departments were
12 competing for these lines.

13 And the assumption that Lynn and
14 Emilio made, which has been borne -- which
15 has been proven over time to be true, is
16 that they could bring people in as
17 post-docs. Many of their post-docs ended up
18 getting tenure-track positions -- and that
19 these people would be so well trained that
20 they could do CSIPEC-type work, which is
21 this highly multidisciplinary work, and
22 still be able to publish in the journals in

1 their disciplines.

2 Now, that may be asking a lot of
3 people. But they've had folks who have been
4 able to do that.

5 MR. BRADY: But I also worry,
6 Cheryl, that's going to end up being very
7 applied, because folks like that, I think,
8 by and large are going to interact on the
9 applied issues. And I know -- it may seem
10 like economics and political science are
11 very close, but certainly when it comes to
12 theoretical issues we haven't necessarily
13 been that close. And some attempts to
14 encourage that would be great, I think. And
15 it's sad of that's thought to be outside the
16 purview of interdisciplinary work, because
17 they're so close to begin with.

18 MS. EAVEY: Oh, I wasn't saying
19 that. I wasn't saying that. I just
20 mentioned their closeness because we were
21 thinking in terms of interdisciplinary work
22 of political scientists with -- and there's

1 a long tradition of political scientists --

2 MS. MORTON: Actually, that's what
3 I used to hear in Iowa and other places I go
4 to, and they'd say, well, we encourage
5 interdisciplinary work but if you work with
6 an economist, that's not really
7 interdisciplinary. Now it may have been
8 they were talking to me specifically, but
9 the idea was that political
10 science-economics collaborations are just
11 not interdisciplinary anymore. And it's
12 really frustrating. I find this very
13 frustrating.

14 I hear this often, that whenever
15 there's interdisciplinary you really have to
16 go work with somebody, you know, who's a
17 biologist or something or it doesn't count.
18 And I -- you know, I think, Richard, that
19 it's great.

20 MR. SCIOLI: Well, for a while at
21 NSF, as a matter of record, there was a
22 tendency toward multidisciplinary and there

1 were even activities that were directed
2 toward bringing individuals from more than
3 one directorate together. So a sociologist,
4 a political scientist, and the psychologist
5 might be, by our standards, an interesting
6 interdisciplinary collaboration, but they
7 would be disqualified from participating in
8 some initiatives at the time because the
9 thought was we want a biologist and an
10 engineer and a behavioral scientist.

11 MS. EAVEY: That still actually is
12 true for some of our across-directorate
13 activities.

14 MR. SCIOLI: And that is a much
15 more difficult hurdle.

16 MR. BUTZ: This hurts other
17 directorates just as it hurts us. This
18 keeps chemists and astronomers or physicists
19 or mathematicians from working together and
20 people across different areas - - computer
21 science and engineering -- it doesn't keep
22 them from working together, it keeps them

1 from getting in mind to work together.

2 MR. SCIOLI: Were you about to
3 comment on the Brookings, Peyton?

4 MR. YOUNG: If you'd like -- a few
5 words about a center that is actually joint
6 between Johns Hopkins and Brookings called
7 the Center on Social and Economic Dynamics.
8 We set it up about 4 or 5 years ago -- 4
9 years ago. Carol Graham (?) is co-director
10 with me. It involves about, oh, I don't
11 know, about eight or nine people at this
12 point, which is a mixture of political
13 scientists and foreign policy types --
14 specifically within political science,
15 several economists, and Rob Axtel, a
16 Carnegie Mellon product, who's a little bit
17 of everything but basically calls himself a
18 computer scientist.

19 As some of you may know, Epstein
20 and Axtel developed this idea of Sugarscape,
21 which they then elaborated into a book
22 called Growing Artificial Societies, an MIT

1 book. It lays out this methodology of
2 dynamical modeling of interactions in which
3 there are many, many agents involved, often
4 with great diversity of characteristics,
5 locations, interaction structures,
6 preferences, all that kind of thing, and
7 shows how you can use this methodology to
8 explore a variety of questions.

9 You're probably asking yourselves,
10 well, what in the hell is Brookings doing in
11 a business like that?

12 SPEAKER: What in the hell is
13 Brookings doing in a business like that?

14 MR. YOUNG: I think there are some
15 people on the board at Brookings who think
16 we should stop this immediately. But there
17 are other people who feel that it's part of
18 a portfolio strategy at Brookings which runs
19 the gamut from -- as you know, it covers a
20 variety of areas topically -- defense,
21 foreign policy, economics, international
22 economics, Congress, and so forth and so on.

1 It's already a rather interdisciplinary
2 place, I mean just speaking by -- and
3 comparing it to Cal Tech, comparing it --
4 it's at least as interdisciplinary already
5 from the get-go. And it's a relatively
6 small place. So there is, I think,
7 basically a culture there which is quite
8 receptive to broad approaches and so forth.

9 Does this have a bearing, this
10 kind of work in the center, on public
11 policy? Well, you know, in an indirect
12 sense some of it does. There's a project
13 that Steinbrenner (?), Axtel, and Epstein
14 are involved with now to try to just get a
15 different sense of how civil disorder, or
16 just how spontaneous events of civil
17 violence can arise from situations that
18 previously were just situations of dispersed
19 dissatisfaction. In other words, there's a
20 quite normal state of the world in which
21 some people are simply unhappy. And then
22 that can suddenly morph, it seems, into a

1 situation where there's a lot of killing
2 going on. And the question is to try to
3 understand both using data and models how
4 those processes occur.

5 That's a very speculative
6 business, obviously. For one thing, it's
7 difficult to get data. However, it turns
8 out that there are some data sets for
9 Central America, El Salvador in particular
10 and Guatemala, in which there are fairly
11 detailed account of killings by village, by
12 actual day, over a several-year period. And
13 so it is possible to sort of mix these
14 data-collection methods, which are of course
15 dispersed in the sense of geographically or
16 spatially dispersed data, to study those
17 event histories and then try to use a
18 computation approach in which you ask, well,
19 what kinds of factors might be entering into
20 the decision making of individuals and can
21 we at least qualitatively replicate what we
22 see going on in the field.

1 So that's an example of a research
2 project which is very much in its infancy, I
3 would say, but it's quite interesting. It
4 involves some of the different -- it's very
5 interdisciplinary, basically. It does have
6 ultimately some insight to public policy, I
7 think. I mean even Brookings understands
8 that. It's a longer-term public policy
9 impact.

10 So that's the kind of work that
11 goes on at the center. We also teach
12 courses at Johns Hopkins. Those courses are
13 very, very popular. I -- Michigan is one of
14 the other places that specializes in this
15 kind of thing, but if you can offer a course
16 at your university in computational models
17 in the social sciences, I guarantee you
18 you're going to have a good sign-up.

19 Now, undergraduates just love this
20 kind of thing. Why? Well, for one thing,
21 they're even more sensitive than we are
22 about the limitations of our disciplines.

1 They keep hearing about rational actor
2 models in economics and suddenly they scream
3 and they say this just doesn't turn me on.
4 And then it turns out there's some course
5 offered by some crazy nut named Rob Axtel,
6 which doesn't assume -- well, it assumes
7 bounded rationality, but then studies a
8 whole lot of questions that they were not
9 exposed to in an economics course, and that
10 really opens up their horizons.

11 So I think these are - there's a
12 lot of opportunity to design new kinds of
13 courses that are very much cross-cutting.
14 Other examples actually are just
15 Brookings-style work brought to the
16 university. Actually this is really
17 interesting. Cliff Gadde (?) has been -- is
18 actually an economist by training but works
19 in foreign policy and in particular in
20 Russian transition to a market economy.
21 He's one of the great experts on what's
22 going on on the ground in Russia over the

1 last 10 or 20 years. And he's able to talk
2 about this both from an applied standpoint
3 and a theoretical standpoint.

4 So he knows what a model is and
5 what a dynamic and a game and a equilibrium.
6 But he also knows what constraints and
7 incentives a, say, manager on a factory
8 floor in the middle of Novo Sibirsk or
9 something faces right now, which is not
10 something that a lot of people do now, not
11 that combination of things.

12 So there's a course at Hopkins on
13 that. Again, it's just -- he has to turn
14 people away, because as academics we don't
15 tend to offer that kind of thing. So these
16 are the kinds of opportunities that I guess
17 are out there waiting to be picked off.
18 It's not that difficult to do it, really,
19 and it's actually not that expensive either.

20 There was one other thing
21 mentioned, and I'm happy to -- I don't want
22 to go on at too much length. But Cheryl

1 mentioned MacArthur networks, that I know a
2 fair amount about. I was going to bring
3 this up as a possible model, somewhat
4 modified, as to how some of the
5 interdisciplinary work that we're talking
6 about might proceed. But that may not be
7 the right -- this is probably not the right
8 point in the agenda to do that.

9 MR. SCIOLI: If you can hold that,
10 there certainly will be opportunity to share
11 that with us and we'd very much like to hear
12 about it.

13 Carl, can you say a word about --

14 MR. SIMON: Yes, thank you very
15 much. I'd like to talk about a little bit
16 what's happening at Michigan.

17 Michigan has had a history of thin
18 walls between departments -- not like
19 Carnegie Mellon or Cal Tech, for sure, but
20 we have a president and provost who are
21 certainly encouraging interdisciplinary work
22 .

1 One of the things related to this
2 group is that the public policy school,
3 where I have an appointment, is just
4 starting a PhD program in which the student
5 has to be either in economics or political
6 science but take a lot of courses in both
7 areas.

8 The center I'm most excited by is
9 the one I'm directing called the Center for
10 the Study of Complex Systems. And I have
11 some handouts on it tomorrow, so I won't say
12 that much about it. I'll try and keep it
13 under 2 hours. Just kidding.

14 Quickly, a complex system I've
15 sort of defined before. It's, you know,
16 where you look at standard social
17 science/biology models and add the things
18 that are missing, like diversity, dynamics,
19 feedback, network, organization, bounded
20 rationality, and see where you can go from
21 there. It almost always requires computer
22 simulation to understand when you add these

1 complexities.

2 We have -- the center's just
3 finishing its second full year as a center,
4 maybe third -- second, I think. And what do
5 we do? Well, we've got weekly seminars with
6 outside speakers, we have a Nobel symposium,
7 which we talk about the prizes.

8 Once a year we meet with the
9 people from Santa Fe and have a week-long
10 workshop on common themes. We also support
11 other workshops; for example, in 2 weeks
12 there's going to be a workshop on pattern
13 formation, a very interdisciplinary one that
14 we're helping organize.

15 We try to encourage
16 interdisciplinary research on complex system
17 approach. So for example, we gave -- there
18 was an NIH -- proposal for an NIH training
19 grant on -- in which students would learn
20 the whole gamut of epidemiology, not just
21 some narrow lab practice but also how to
22 build a model and how to use a model --

1 again, somewhat related to what we're
2 talking about. And we funded a couple of
3 the graduate students to see if we could
4 talk NIH into funding this.

5 We're starting something called
6 complex systems and X conferences, where X
7 will be different topics. Our first one, I
8 think, will be traffic flow.

9 We have a computer lab where most
10 of the university interacts with us, so we
11 do -- most of the university will do complex
12 systems approach, so we have Rick Riolla
13 (?), who -- a magnificent research
14 associate, teaches courses and develops the
15 software for complex systems.

16 We're developing relationships
17 with local industry. It turns out Ford has
18 a crew of about 26, 27 people doing this
19 kind of work. General Motors, we just
20 learned, has three to 10 people. And we
21 just got them to meet each other to meet for
22 the first time, through our network. So

1 what we do is have meetings at each other's
2 places, that we organize.

3 We have a certificate program in
4 which a graduate student can take five
5 courses and, with some flexibility, get a
6 certificate in graduate complex systems.

7 One of the exciting things is, as
8 part of our setup, we were given some
9 positions, and our first one was Scott Page.
10 Basically, the deal was that we could
11 hire -- we have to convince a department to
12 hire the person, but the provost would pay a
13 quarter to half that person's salary
14 forever. And our second hire is Mark Newman
15 (?), who's a Santa Fe post-doc who does
16 network theory -- very interesting to social
17 science -- but he's a physicist. So this is
18 a very exciting hire.

19 You'll see the list. One of the
20 other things exciting things we do is try
21 and encourage interdisciplinary grant
22 proposals. In some ways we were set up to

1 do that, and our money to get us going was
2 thought of as seed money. So for example,
3 we did put in the biocomplexity RFP for
4 something in which we would show that how
5 networks -- how context structures and who
6 encountered whom affected outcomes.

7 We did one with ITR about thinking
8 that, really, computer scientists and social
9 scientists care about the same things, like
10 protocol, organization, decentralization --
11 let's get them together with post-docs and
12 students to sort of formalize this.

13 We had the IGERT that I mentioned,
14 we had an NIH training grant that I
15 mentioned. None of these have worked yet,
16 and the provost who started our funding is
17 going to the University of Illinois -- lucky
18 you. She was great. And so there's a
19 little concern about where we're -- how
20 strong we'll be over the next few years.
21 Certainly there will be big -- some
22 cutbacks, but we'll be there and, hopefully,

1 fostering the kinds of things we're talking
2 about here.

3 The other place, let me just
4 mention Santa Fe Institute, since that was
5 on the list. I try to go there somewhat
6 regularly. They care about many of the same
7 issues, maybe a little broader. We of
8 course have physicists, biologists, and
9 engineers associated with us. But Santa Fe
10 has Nobel laureates in those areas, and they
11 sort of set the tone, for better or worse,
12 on complexity.

13 As Peyton mentioned, or hinted to,
14 some of the Nobel laureates can go pretty
15 far out on the limb and talk about what's
16 really cutting-edge research -- some of it
17 works and some of it doesn't. But it's an
18 exciting place to be.

19 They have post-docs there. They
20 have no regular faculty and no educational
21 component. Part of our dream in our own
22 Complex Systems Center is that we might be

1 part of their educational component. But
2 certainly I go to two or three conferences a
3 year there. They're always very
4 interdisciplinary, and to me, I've never
5 come back without being very excited about
6 the project that I just heard about.

7 I don't know what more I can say
8 about Santa Fe, but, you know, it's just
9 getting -- its definition is
10 interdisciplinary, but in a notion of going
11 beyond -- modeling is important, by the way,
12 for both Santa Fe Institute and certainly
13 for the Complex Systems Center, modeling is
14 at the core.

15 I've also been to CSIPEC. Lynn
16 and I have some papers coming out of the
17 CSIPEC, and boy, does that work well.

18 MR.GRANATO: Okay, what we're
19 going to do now is go to discussion point 4.
20 The purpose of the remainder of our time
21 today is going to be setting the table for
22 tomorrow morning. It's 4:00, so why don't

1 we take another 10-minute break, and then
2 we'll -- I mean, would anybody object to us
3 going to just 5:30? We'll come back to
4 discussion point 4 and we'll finish up.

5 (Recess)

6 MR. SCIOLI: Let me just set the
7 kind of parameters for where we're headed.
8 First, I hope you agree that it's been
9 useful hearing war stories. Because we find
10 it very useful. They seem like they're
11 peace stories as opposed to war stories.
12 But I invited Jim Alt to say a word about an
13 evolving activity at that stodgy place
14 that's kind of interesting and unique. But
15 Jim and I certainly don't want to preclude
16 anyone else from telling a success story.
17 Don't feel that we're singling out the Cal
18 Techs and the Carnegie Mellons, because you
19 might all have gone through these battles.
20 I guess Becky said, you know, well, the
21 "normal" department. We kind of look at
22 many of you as being in normal departments,

1 but that's good. It's good to have a normal
2 king.

3 In any event, Jim, would you mind
4 telling us about your lapse of sanity when
5 you agreed to create this activity?

6 MR. ALT: Most of you know
7 something about us, you've been to visit, so
8 I'll just take this chance to extemporize,
9 because I didn't know until about 10 minutes
10 ago that I was going to be doing this.

11 A little bit about what we do --
12 when I say "we," I'm talking about the
13 Center for Basic Research in the Social
14 Sciences at Harvard -- CBRSS. Pronounce it
15 "sea breeze" and understand that it is meant
16 to be a breath of fresh air in a place, as
17 Frank said, that can sometimes be, well, a
18 trifle stuffy.

19 Basic research means basic
20 research. It means not applied research.
21 It means our focus is on innovations in
22 theory and methods. Conceptually we

1 organize ourselves into kind of broad-gauged
2 initiatives of which I think there are
3 probably about four -- the human security
4 initiative I'll say a couple of words about
5 in a minute; a general interest in the
6 microfoundations of social science;
7 empirical methods; and positive political
8 economy are probably the easiest way to get
9 the length and breadth of what we try to do.
10 Within those initiatives we have research
11 activities, collaborative activities,
12 residence programs -- all the usual stuff
13 and some unusual stuff.

14 Some of the most important things
15 we do are provide post-doctoral
16 opportunities and visiting scholar
17 opportunities. It wasn't until my third
18 reading through the gray book, which is the
19 Harvard procedures for academic appointments
20 manual, that I discovered that the
21 difference between a post-doc and visiting
22 scholar is that post-docs work on someone

1 else's research and visiting scholars work
2 on their own. So you actually have to tell
3 Mother Harvard what you're doing when you
4 come there to be appointed.

5 I think actually, having
6 discovered that distinction, it's really
7 important. Because I now use the word
8 "post-docs" in the sense we've been using it
9 in this meeting actually to mean what
10 Harvard means by "post-docs" -- the idea
11 that you would go for a year to work on
12 someone else's project and learn from that
13 experience, rather than having, for example,
14 a year at the center at Stanford to, you
15 know, write your next book or something like
16 that.

17 And I think it's important that we
18 try to do that. We do graduate student
19 support, undergraduate research
20 opportunities, things like that. We can
21 talk about them later.

22 A lot of conferences and

1 workshops. Those who are close to us know
2 that we've now done three twice-a-year
3 experimental methods conferences in which we
4 bring in people from psychology, behavioral
5 economics, various social sciences to give
6 talks which are sort of a blend of substance
7 and method.

8 Tommy Palfrey was actually our
9 very first speaker at the first conference.
10 And I think in November we're going to have
11 the next one, and it will include people
12 like Tom Gilovich (?), a psychologist from
13 Cornell, George Lowenstein from Carnegie
14 Mellon.

15 So these are pretty broad-gauged
16 things. They talk about current research to
17 expose, you know, students to that,
18 technical innovations -- we had a session on
19 Internet-based experiments, by which I don't
20 mean setting up a Web site and having people
21 push buttons, but actually using the
22 Internet to run the experiment at a remote

1 site -- actually having students in a lab in
2 South Carolina. But, you know, once you
3 have that as a node in a network of
4 experimental centers, those experiments
5 could have been going on anywhere.

6 And it was kind of interesting to
7 see the technology come along. We've had Al
8 Roth and Dan Gilbert, Max Baselman (?) and
9 others debating the ethics of paying
10 subjects and things like that, so it's just
11 a -- not a broad-gauged view.

12 We're following that up with a
13 teaching experiment this summer. Becky
14 Morton has kindly agreed to give up four or
15 five days of her life to teach two-a-day
16 sessions to graduate students who know
17 nothing about experiments. And the Business
18 School was kind enough to give us access to
19 Al Roth's lab for a couple of sessions so
20 the students could actually see an
21 experiment being run and maybe even be
22 subjects themselves -- are you going to try

1 to do that? So, you know --

2 MS. MORTON: And learn how to
3 program some.

4 MR. ALT: Yes. A little hands-on
5 experience. And if this works well, I'm
6 going to bring the experience back here, you
7 know, to talk about trying to build
8 something like this into the foundations of
9 a methodology group, so that the political
10 methodology group will have to rename itself
11 the statistical methodology group and the
12 new political methodology group will
13 actually include statistics, experiments,
14 and computational modeling and all the other
15 things that we do.

16 Having just hired Lars Erik
17 Sieberman (?) in the department at Harvard,
18 we offered him financial support to teach a
19 pioneer course in computational modeling
20 this year. And that was very successful, by
21 the way. I agree, boy, people beat the door
22 down for those things. Wow. So that's now

1 in the curriculum.

2 And I don't know, we do stuff like
3 that. We have this series called Encounters
4 With Authors, in which we get people who've
5 just about finished a book to come in for a
6 week and we bring in graduate students and
7 junior faculty from around the country to
8 read the manuscript in advance and then
9 critique it while there's still a chance of
10 having an impact on the authors. Person and
11 Tabolini (?) did their book a couple of
12 years ago, and we had John Huber and Chuck
13 Shipman (?) bring their new comparative
14 study of delegation in this summer. These I
15 think are very good formats for just
16 generally broadening horizons of scholarly
17 communication.

18 We also foster research. The
19 first project we tried to kick-start began
20 as an investigation into neural network
21 methods broadened out into something called
22 military conflict as a problem of public

1 health, and is now human security. It
2 started one day when one of us heard a
3 spokesman say, We were surprised by the
4 magnitude of the refugee problem in Kosovo.
5 And it struck me that no one should be
6 surprised by that anymore, because it's sort
7 of folk wisdom that military strategy now
8 targets civilian infrastructure and the
9 consequence of the conflict is always likely
10 to be, in circumstance like that, a massive
11 refugee problem, and that perhaps the
12 problem was a lack of infrastructure for
13 forecasting the actual probably human costs
14 of a conflict.

15 And now you can see how we
16 generated an interdisciplinary project that
17 involved statisticians to build better
18 forecasting models, international relations
19 scholars who supposedly know something about
20 the substance of the causes of war, and
21 public health scholars who have the ability
22 to turn those forecasts, costs, and

1 consequences into measurements of human
2 suffering and emiseration. And the project
3 is now in its third year and moving along.

4 We just got some support from the
5 NSF -- thank you, Frank -- for a second
6 project completely independent, hung on the
7 digital library project in the Virtual Data
8 Center, under NSF's DLI initiative, to
9 design a feasible citation standard for data
10 sets. This is something we think is long
11 overdue.

12 I think the best way to understand
13 the intuition for this project is everyone
14 thinks online access solved the problem of
15 getting access to data and replicating
16 studies, but in fact it just made it 10
17 times as bad as it ever was because Web
18 sites come and go with a half-life
19 measurable in weeks. Data sets change all
20 the time. And there's no way to discover,
21 if you try to replicate something by
22 grabbing someone's data off the Web, if

1 you've actually got the data set they used.
2 And we proposed to tackle this as a
3 theoretical sort of cataloging problem, to
4 design a feasible standard for identifying
5 data sets uniquely so that those who want to
6 replicate others' research can at least know
7 at the point that they begin that they
8 actually using the actual set that generated
9 the results that they're trying to follow
10 up.

11 And we have other projects in
12 mind, but I've talked long enough, so --
13 that's us, that's what we do.
14 Www.cbrss.harvard.edu. We try to keep
15 everything we do somewhere online, so just
16 think of sea breeze, think of that breath of
17 fresh air. Come breathe it occasionally.

18 Okay, Frank, is that what you
19 wanted?

20 MR. SIMON: Is there a mailing
21 list or something that we could keep track
22 over time?

1 MR. ALT: We don't actually have
2 formal mailing lists. If you are interested
3 in one of our activities, the Web site
4 should any day now tell you how to get in
5 touch with the organizers and stay in touch.
6 But it's a pretty transparent Web site.

7 The other thing I should say is
8 that, boy, having been introduced in the
9 line of war stories, I have many. Ask to
10 see my rooms at dinner tonight. I never
11 knew it could take this long to set
12 something up and get it going.

13 MR. YOUNG: Just broadly, where
14 does your funding come from?

15 MR. ALT: Wherever I can get it.
16 We were given a checkbook, we were given a
17 kind of long-run matching scheme by which we
18 get grants and we earn endowment credits,
19 and the endowment credits eventually turn
20 into endowment income. Though under the
21 Harvard formula, that takes awhile.

22 It is really true, I said this

1 earlier, but I never understood so clearly
2 before now how an institution amasses a \$26
3 billion endowment. They do it by -- they
4 end up doing anything, and using other
5 people's money whenever possible.

6 MR. SCIOLI: So have you guys
7 bridged the divide between formal and
8 empirical?

9 MR. ALT: Well, the divide doesn't
10 bother us. You know, in a way that's a nice
11 segue to the topic that we're supposed to
12 turn to, because it seems to me the best
13 reason for having this meeting is that, you
14 know, we sort of spent the morning saying
15 the divide's not a problem and it's a
16 problem of science not a problem of
17 political science, blah, blah, blah, blah,
18 blah. But right now in political science it
19 is a problem because the conflict that you
20 sort of sense, the divide between formal
21 modelers and empirical modelers is keeping
22 them from presenting a stronger and more

1 united common front to the perestroika twins
2 and, you know, the divide that does exist in
3 a lot of departments.

4 And that's a problem. I mean, we
5 do not want to be two contending factions
6 separated by a common interest, to
7 paraphrase Churchill. And I think there
8 is -- if there is a problem in the field at
9 the moment, it is that -- meetings like
10 this, and we're all pretty much in
11 agreement. I can run a center like CBRSS
12 and nobody argues with anybody. We're just
13 pretty happy about, you know, broad
14 priorities and, you know, and you take
15 turns, you take other turns, and it -- you
16 know, it's not that hard to keep the peace.

17 But in the discipline at large and
18 in departments at large, I'm not sure that
19 the people -- it is certainly clear in my
20 department that the many people who do not
21 do formal work or empirical modeling, we're
22 all just one object at the distant end of

1 the department. But it's not clear that
2 among ourselves we act as though we were all
3 the time. And I think that's a problem.
4 And, you know, that's a good way to start
5 thinking about some of these activities for
6 the future.

7 MS. MORTON: Yes, going back to
8 this discipline as a whole -- and you kind
9 of asked us about, like, what happens when
10 get back to our departments, is -- this
11 happened at Iowa a lot. Faculty would tell
12 graduate students -- and of course they
13 would never tell -- I could never figure out
14 which faculty members were saying this --
15 that the students should either take the
16 methods sequence or the formal modeling
17 sequence or neither, but not do both because
18 that would be just too much and they
19 wouldn't get enough substantive courses.
20 And there really -- there was this pressure
21 against doing both of these things, you're
22 not getting enough substantive stuff.

1 And I don't know -- I mean, there
2 is this large community that's not
3 represented at this table, and very many
4 people are like one or two people in our
5 department, with the rest of the people
6 being -- going around saying don't take
7 courses, you know, that do that.

8 MR. SCIOLI: Well, there will
9 always be the unwashed.

10 MS. MORTON: Yes, but they're the
11 majority in a lot of places.

12 MR. SCIOLI: Well, that's why we
13 have Jesuits. Right, Henry?

14 MR. BRADY: Absolutely.

15 MR. SCIOLI: There have to be best
16 practices, though, that you can suggest
17 you've learned from that would give us
18 incentive at NSF to jump-start, if you will,
19 or to move along slightly or to even push
20 along. Because at the beginning of the day
21 we talked about not simply political science
22 -- this not being simply a problem for

1 political science, but this being a problem
2 for science writ large. And Jim and I talk
3 almost every afternoon about this as a
4 problem for science, and challenge each
5 other to try to think of examples where this
6 kind of a divide does not exist, and why it
7 doesn't exist -- or if it doesn't exist, why
8 it doesn't exist.

9 So what are practices that you can
10 think of? Is it collaborative research
11 teams, where perhaps it's too much to ask a
12 graduate student to take formal theory and
13 empirical research -- and Dick mentioned
14 parenthetically adding a year to the program
15 at Cal Tech. But is it bringing three
16 different hats to the table -- the
17 substantive person, the formal person, and
18 the empirical person? Now, I can think of
19 projects that we've supported, and I know
20 that strange combinations around this table
21 have taken place.

22 MS. ZINNES: I think it depends on

1 whether you're talking about research or
2 training. I mean, if you want to stimulate
3 research enterprises that combine these two
4 streams, I think Peyton's idea of holding a
5 competition is excellent. And it should be
6 substantively based -- start with a couple
7 of questions, and then have a kind of a -- I
8 don't know if it would be a competition in
9 terms of writing grants, exactly, but maybe
10 several --

11 I mean, we've talked at Illinois
12 about doing something that we call a senior
13 master class, where we would actually take
14 somebody's not formally stated but
15 potentially formally possibly stated
16 argument in some new piece of research that
17 they're working on. I mean, some -- you
18 know, John Vasquez (?) did some interesting
19 work on territory and conflict and so on.
20 Take something like that that's really
21 meaty, bring somebody in like that, and have
22 a bunch of graduate students who have been

1 working with those questions but also who
2 have been trained in both the formal and
3 statistical areas take a stab at the
4 question.

5 How would you take the question
6 of, you know, the importance of contiguity
7 as a factor in conflict -- you know, how
8 would you set that up as a problem and set
9 up a model and possibly test it? And then
10 bring somebody in like Vasquez to suggest
11 whether or not you're way off track or not.

12 But I think that sort of thing
13 where -- I don't -- I think we need to be
14 really problem based. I really think we
15 need to be substance based. We need to have
16 the questions first and then bring together
17 the people who are interested in the
18 questions.

19 That's why I think one substance
20 person, one methods person, one modeling
21 person just won't work very well. First of
22 all, the substance person typically can't

1 speak to the other two people; and secondly,
2 I don't know why methods people are divorced
3 from having substantive problems. Henry,
4 you were saying this morning that you were
5 reduced to having to teach methods for the
6 first 10 years of your -- and why should
7 that be the case?

8 We went into political science not
9 to be methods people, we went into political
10 science because we liked problems in
11 political science. And I think those things
12 should be there all the time.

13 And I think if you go to the
14 student level, and that's a whole different
15 issue, how you train the students, but I
16 think you have to sort of separate those.

17 MR. YOUNG: Well, except -- there
18 was a time when I taught statistics to
19 first-year graduate students. Actually,
20 they were in public policy not in political
21 science, but I would guess that a similar
22 strategy would work in political science.

1 And I don't know how I got onto this, but at
2 any rate, I put together a series of what
3 amounted to cases, really hard cases.

4 One of them, just to illustrate,
5 was does eating eggs -- or too many eggs
6 raise your risk of having a heart attack.
7 It was much in the news at the time. Eggs
8 were thought to be bad, eggs had
9 cholesterol, cholesterol is bad -- you know,
10 the whole thing. And we spent about 3 weeks
11 on just that question. And I used it as a
12 vehicle to teach statistics.

13 And it's a very, very tricky
14 problem as it turns out, with all kinds
15 of -- there's the Framingham study, there
16 are many famous data sets that can be
17 brought to bear on this. There's no lack of
18 data. The question is what's the
19 appropriate method and inference to use to
20 draw a final conclusion.

21 Anyway, I raise that -- I point --
22 how you construct a nifty course that is in

1 a sense problem based, but you can use it to
2 teach methods. But it's a different way of
3 teaching.

4 MR. STRAF: Could I ask if
5 political science uses formal methods of
6 research synthesis, like meta analysis or
7 something like that? Is it known or --

8 MR. ACHEN: There have been some
9 examples. The problem, I think, with some
10 of the skepticism about it is that you're
11 often averaging over 15 studies, 12 of which
12 shouldn't be taken seriously to begin with.
13 And the other three may be the ones that
14 come to the opposite conclusion.

15 MR. STRAF: But there's at least
16 bodies of studies that one could say are
17 similar in their design or --

18 MR. ACHEN: Usually, no, they're
19 not that similar. We haven't done very much
20 of this. It was tried a little bit by a few
21 people.

22 MS. ZINNES: In what area?

1 MR. ACHEN: I'm trying to remember
2 now. Henry, do you remember? There have
3 been a couple of meta analysis things, and I
4 can't remember now.

5 MR. BRADY: I just can't think of
6 any. Maybe there have been, but we're
7 remarkably free of that kind of careful
8 thinking about what we've amassed. Which is
9 a problem. I mean, it's really interesting.
10 When I started doing research on welfare,
11 which I do as sort of my hobby these days,
12 it was amazing to me to find out that people
13 actually cared about the result.

14 I found myself saying, gee, you
15 know, it really matters to do this right in
16 a way that when I did political science and
17 would worry about getting it right, it was
18 clear to me that nobody ever really cared
19 very much and that the important thing was
20 just to get a publication. I just don't
21 think that we take ourselves seriously
22 enough or the discipline doesn't or

1 something doesn't, because we're not
2 tough-minded about it.

3 MR. SIMON: For me that was
4 exactly the lure of doing epidemiology. I
5 mean --

6 MS. ZINNES: Because it matters.

7 MR. SIMON: It mattered. People
8 cared how contagious HIV was or --

9 MR. BRADY: And it's a very
10 disciplining kind of thing when you say it
11 really matters to get it right. And I can't
12 B-S my way through it.

13 MS. MORTON: One think I think
14 that prevents this kind of meta analysis is
15 a lot of what we do -- well, there are a
16 huge number of people using the same data
17 set in different ways, you know, and
18 especially in, say, the voting literature
19 and public opinion. And so we're all taking
20 this same data set, looking at it
21 differently here, differently there. Maybe
22 we add a variable that we get from some --

1 we combine data sets.

2 But it's not like they're a study
3 here and a study there and a study there
4 that you can do this analysis. What happens
5 is you have a history of studies where
6 somebody looked at the data set and found
7 this, and then somebody looked at this data
8 set and added something and found that. And
9 so it does -- the next step is just to go
10 back and look at that data set and then do
11 something else with it. And there's a real
12 problem with that fact that we're very much
13 driven by these huge data sets that we have,
14 that we kind of get tied to.

15 MR. SCIOLI: The argument that we
16 hear all the time, and you heard it, I'm
17 sure, Becky, is that these large -- the
18 large data sets across sociology, political
19 science, economics, et cetera, are then
20 public goods. And there's an economy to
21 creating the data set and have numerous
22 scholars mining it independent of the fact

1 that, you know, if there's a careful job in
2 planning the collection of the data.

3 MS. MORTON: But I mean, I'm just
4 saying I think that's why we don't have this
5 meta analysis is because we sort of are
6 doing it with mining the same data over and
7 over again. We're not getting -- you know,
8 the meta sort of stuff we're talking about
9 is like a study here and a study there,
10 right? And we're kind of looking at them
11 combined.

12 You're talking about the kind of
13 thing that recently came out that said that
14 placebos don't work? I mean, wouldn't that
15 be the kind of meta analysis that --

16 MR. STRAF: I was thinking of it
17 as a formalized research synthesis, a --
18 more or less of an indicator of, first of
19 all, how problem-oriented the field might
20 be, how much, you know, of common designs
21 are used.

22 MR. BRADY: Actually the one place

1 where there's been something like meta
2 analysis has probably been the election
3 forecasting literature. There's a nice
4 article in PS by Larry Bartel and John
5 Zeller (?) which does at least model
6 averaging across a bunch of different
7 models, and that's interesting. And there's
8 actually -- I think in the last 10 years
9 there's been some pretty tough- minded
10 thinking about what are reasonable
11 specifications.

12 The problem that you're talking
13 about, a data set with, what, since '48
14 basically, so it's got 25 observations and
15 we got about 30 right-hand side variables
16 you can think of at least, work it out.
17 There's probably a problem there.

18 MR. ACHEN: At least there was
19 scientific consensus. They all agreed who
20 the president was going to be.

21 MR. BRADY: Going to be, right.
22 But what I like is that the Bartel-Zeller

1 article after the fact shows how they could
2 have predicted better. I wrote to Larry, I
3 said "very impressive prediction after the
4 fact."

5 MR. BRADBURN: Once
6 experimentation takes over you have lots of
7 opportunity to do better after the fact.

8 MR. BRADY: Right.

9 MR. BRADBURN: Because one of the
10 problems with them is you have lots and lots
11 of discrete experiments, each one of which
12 is a little different or -- ostensibly
13 dealing with the same topic, so combining
14 the datas. It's a different world then.

15 MR. GRANATO: One of the things,
16 to put a little structure in this, it seems
17 to me that we're going to have to separate
18 long-term effects from something to be done
19 in the immediate near term.

20 And we're faced with departments
21 that have tenure. In many of these
22 departments tenure is abused. And so what

1 can be done that -- I mean, people that do
2 the work you do I think are in the distinct
3 minority in most of -- when you get out of
4 the top ten, you're not going to see the
5 kind of work you do, for the most part, in
6 these departments. And there's going to be
7 a cadre of people that are going to try and
8 prevent this.

9 So with that type of constraint in
10 mind, what can be done in the short run to
11 create a growing body of people that do this
12 type of work, where the evidence that change
13 is coming will be in the syllabi and the
14 type of courses that are offered? Like in
15 computational methodology, we don't see that
16 in most departments.

17 So in terms of delineating
18 initiatives -- wrong word. In terms of
19 initiating -- in terms of discussing program
20 priorities in a Dear Colleague letter from
21 this, our directorate, what can be done to
22 do that?

1 MS. ZINNES: I think you almost
2 have to turn the question around a bit,
3 because it seems to me we could sit here and
4 dream about a whole lot of things that one
5 would love to do. I think the issue is what
6 is it that NSF can do. That is, what are
7 the things that are within your purview that
8 you can actually influence? I mean, you're
9 not going to change departments, you're not
10 going to keep people from being hired that
11 object to this form of research. I mean,
12 you're not going to stop tenure. Those are
13 all reality.

14 So the question is what is it that
15 a funding agency like National Science
16 Foundation can do. And I think those are
17 sort of straightforward. You fund graduate
18 students in terms of fellowships. You fund
19 conferences in terms of getting people to
20 talk to each other. You may be able to fund
21 some sort of -- although, I gather, not
22 terribly much -- some sort of educational

1 mechanism, but maybe through workshops,
2 summer workshops.

3 MR. GRANATO: Could I stop you
4 right there? If you wanted to fund a
5 graduate student -- I mean, you've already
6 done it at Cal Tech. You've extended a year
7 in the program. I mean, would a good idea
8 be one in which we give not just
9 dissertation fellowship support, but we give
10 an extra 2 years or something to a graduate
11 student to extend their training within the
12 program that they're already in. Is that
13 the kind of thing that might work?

14 MS. MORTON: Yes. I think that's
15 a --

16 MS. ZINNES: That would be a step
17 in the right direction.

18 MR. GRANATO: And it's not going
19 to be too expensive, I don't think, either.

20 MR. McKELVEY: The problem with
21 that is how do you do that without sending
22 the wrong signal to the market. I mean, one

1 of the things that people look at when you
2 come on the market is how long you've been
3 in graduate school.

4 MS. ZINNES: Yes.

5 MR. ALDRICH: I was thinking sort
6 of setting it up -- actually I think there
7 was once a conversation with SSRC people
8 that, you know, the normal SSRC thing was to
9 send people out to the field and let them
10 learn their country. Then instead of doing
11 that -- was to have them learn theory and
12 methodology. And so one possibility would
13 be visiting -- a year visiting at Cal Tech,
14 Rochester, whatever, in, say, immediately
15 post-exams or something like that, before
16 they write their dissertation.

17 It would signal that by, you know,
18 NSF-sponsored field research in theory and
19 methods, then I think it at least accounts
20 for that year in a way that, you know, just
21 saying, oh, you stayed an extra year, took
22 longer, and so you were slower in getting

1 out would --

2 MR. YOUNG: SSRC did a program for
3 several years in economics at Airlie House
4 out here in Virginia. It was a short -- I
5 can't remember. It was about a 2- or 3-week
6 course. And they just wheeled in a number
7 of -- it would be the equivalent in
8 economics of inviting the people around this
9 table essentially to each give a couple
10 hours lecture introducing graduate
11 students -- these were typically students
12 that were close to being done -- well,
13 third-year students or up. And it was just
14 a way of getting them exposed to the
15 literature. It wasn't a training course per
16 se, it was simply allowing them to be better
17 informed about some of the alternative
18 approaches.

19 I think -- well, let's see. I
20 think that program has actually stopped
21 now -- it would be interesting to find out
22 -- because there was some changeover at SSRC

1 in the management, and whoever was
2 responsible, that person left. But I think
3 on the whole it was regarded as quite a
4 success. It was very low-budget. I mean,
5 people were not -- the professors who came
6 in often did so for -- I mean, I'm not sure
7 that there was any honorarium, or if so it
8 was very nominal. But at the same time it
9 created a culture, you know, of -- and it is
10 important to get to these students.

11 I must say that I'm a little more
12 skeptical about -- well, fellowships in the
13 usual sense. I think that those can be
14 garnished, you know, sort of reoriented. We
15 all know how to do this, right? A student
16 gets a fellowship to do X, but then by the
17 time you're done with him, it's X- prime.
18 And, you know, you fool around with it. And
19 it really isn't serving the purpose that NSF
20 might have wanted. It just gets rerouted.

21 MR. GRANATO: What John said,
22 would that be okay from your -- in terms of

1 the market signal, in terms of --

2 MR. McKELVEY: Yes, I think maybe,
3 you know, post-doc, but specifically 1- or
4 2-year post- doc. I mean here again you
5 have to make sure you get students who are
6 the best students. You know, I mean,
7 because even now with post-docs, frequently
8 it's not the best students who end up with
9 post- docs. The best students will get the
10 -- they sort of go out and get top jobs.
11 But I mean if you had really attractive
12 post-docs which would be NSF- supported and
13 have some kind of prestige to them, I think
14 that would -- maybe something like that
15 would --

16 MR. ALT: I think that's a very,
17 very important point. There really is a
18 tendency to see post-doc on a vitae and
19 think, oh, you didn't get a job last year.
20 So looking ahead to tomorrow, talking about
21 writing a letter to colleagues, one of the
22 things you can say to colleagues is we're

1 going to do this and we're going to do this
2 for strong students. And in order for this
3 system to have any prayer of working, you're
4 going to have to start writing, you know,
5 job ads that say preference given to people
6 with NSF post-doctoral experience. And then
7 students will want them, people will want to
8 hire them, and projects will be glad to have
9 the people associated with them, and it'll
10 work.

11 MS. MORTON: Well, I think part of
12 the key is how it's funded, right? I mean,
13 the Robert Wood Johnson thing doesn't have
14 any negative -- I mean, it's a very
15 competitive thing. The people I know who've
16 gotten it think it's just absolutely
17 wonderful, and it does look wonderful. And
18 I think the key is, is that they're making
19 the same amount of money they would be
20 making at a regular job, right?

21 So if it's a post-doc and it's
22 paying less than you would get in a regular

1 job, then people would be tempted not to
2 take it and then you would get the
3 less-capable students. And so, yes, I think
4 when you're looking at this Robert Wood
5 Johnson thing and how they manage to make
6 that such an -- because it is a very
7 attractive thing. And I know lots of people
8 who are very good who applied and didn't get
9 it. And I -- but you know -- so they seem
10 to be very successful.

11 MR. SIMON: Let me say something
12 about the Robert Wood Johnson. I've been on
13 the Michigan advisory council for them. One
14 reason they work is that there are just two
15 or three place where -- you know, Yale,
16 Berkeley, and Michigan -- where one can go,
17 and there's a real structure nourished
18 there.

19 Each place has a very dedicated
20 leader making sure it works. And it works
21 so strongly that these students not only --
22 the post-docs not only come out with good

1 jobs, but they've been diverted from the
2 mainstream and they still come out with
3 great jobs in some ways, right. I mean
4 they're no longer pure political scientists;
5 they're now health economists and health
6 political scientists. And I think that
7 makes it a little tougher in some ways.

8 But we've been talking about
9 post-docs, and I think a good question is
10 who will -- custodiate custodes, you know,
11 who's going to take care -- who's going to
12 be in charge -- docking the post- docs?

13 MR. BRADBURN: Well, that's -- on
14 the Johnson model. There are two different
15 models for post-docs. One is like our
16 graduate fellowships, you just fund
17 post-docs and they go wherever they want to.
18 And the interesting thing about the Johnson
19 one is they have a competition among
20 universities for the programs. So they're
21 funding the programs at the universities, so
22 it means that the institution has to have

1 people there who really want to run this
2 program and have the post-docs there. So
3 it's -- and that's very competitive among
4 institutions. So it's -- it does -- and
5 they build in things like advisory groups
6 and so forth to kind of oversee them. But
7 again, they're very rich programs, so to
8 speak. My understanding, I think, is they
9 run the competition for the post-docs --
10 they select their own post- docs.

11 MR. SIMON: Right. Each -- well,
12 they select them but Johnson actually makes
13 the final decision about who goes where.
14 But the students, for example, take an
15 intense set of courses their first semester
16 on health --

17 MR. BRADBURN: Usually -- there
18 are two -- actually, there are two different
19 Johnson programs. One is to get social
20 science, PhDs in social sciences to be
21 interested in health issues. The other
22 one's the opposite; that is, to get MDs to

1 be trained in social science. I've been
2 associated with — on that side. But
3 again, they have the same structure. That
4 is, they have a competition among
5 universities for the programs, and they fund
6 them very well.

7 And then there's this very funny
8 competition. It's a double competition for
9 the students, because the students apply to
10 the program, and then -- at least the
11 medical ones, which I'm more familiar
12 with -- then they decide which -- I think
13 there are nine programs on the medical one.
14 They decide which of the nine they really
15 want to apply to, but they have to apply to
16 three or four. They go around and are
17 interviewed by all these, and then there's a
18 sort of joint -- it's like internships, you
19 know, sort of a joint ranking. The student
20 ranks the ones they want, and the
21 institution ranks the ones they want, and
22 then the foundation sort of plays a kind of

1 matching game in which they try to maximize
2 the overlap between things. So -- I don't
3 know if that's the way they run it in the
4 social sciences --

5 MR. BRADY: That's the way it
6 works.

7 MR. ALDRICH: How many students
8 does each school have to take each year?

9 MR. SIMON: Four.

10 MR. BRADBURN: Well, in the
11 medical ones it varies.

12 MR. SIMON: I see. Well, four,
13 and it's got to be an economist, a political
14 scientist, and a sociologist, and a -- you
15 know.

16 MR. BRADBURN: I must say from a
17 funder's point of view, they're
18 extraordinarily inefficient. I mean, they
19 put a tremendous amount into the program for
20 very few students. It's wonderful for the
21 students. But when I think about the
22 resources that we put up for these four

1 students or something, is just mind-boggling
2 how --

3 MS. MORTON: You mean in terms of
4 staff or --

5 MR. BRADBURN: Yes. I mean, the
6 faculty that's devoted to this is -- and
7 paid. Johnson's putting, you know,
8 reimbursing the universities for a big chunk
9 of time for these people to really mentor
10 and train these people. And it's a great
11 training, but it's very expensive.

12 MR. FREEMAN: I don't think
13 there's necessarily a choice between getting
14 a job and getting a post-doc. We hired
15 someone a couple of years ago from Columbia
16 and he secured a large post- doc from a
17 project -- a — states project, actually,
18 and we gave him an offer and the option
19 whether to exercise it or not so he could go
20 off and work for a year on this other
21 project. And then he came last fall. And I
22 think one of Jim's students went to the Bank

1 of England --

2 MR. ALT: Absolutely.

3 MR. FREEMAN: And had a wonderful
4 Ivy League offer from --

5 MR. BRADBURN: We actually hired
6 one of these Johnson fellows, but she took
7 the fellow -- so we postponed the beginning
8 of the -- this was a professorship -- till
9 she finished the -- or actually for 2 years.
10 It was a kind of mix. She came for
11 awhile --

12 MR. FREEMAN: Can I make a
13 different point, though -- I think one thing
14 I'm worried about is we did this before --
15 and I wasn't going to say this till
16 tomorrow, but -- I hope you talk to -- I
17 have three people in my memo, three young
18 people for whom I have tremendous
19 admiration. And I just think they're doing
20 some of the best work in the country in all
21 respects. One of them's name -- John
22 Londregan's been mentioned, so I'll just

1 mention -- I hope you -- you should talk to
2 them too.

3 I'm a little nervous about sitting
4 here on the 12th floor saying let's design
5 this competition or this ideal scheme for
6 which these people are going to come out of
7 the -- you know, rise up and make their
8 proposals and win the competition and so on.
9 I think it might be best just to ask some of
10 the people -- identify who we think are
11 really some of the people on the cutting
12 edge who are doing the best work, and then
13 -- you guys need to talk to them and say
14 what do you really need to make sure this
15 work gets done.

16 MR. ACHEN: It's ——— though. We
17 can be spared.

18 MR. FREEMAN: The worst thing
19 would be to have a competition and have the
20 people who win it -- then Frank says guess
21 who won these post-docs and these grants,
22 and we say, oh, God, you're not going to get

1 a lot out of that investment.

2 MS. MORTON: Is this off the
3 record? Don't give anything to —.

4 MR. FREEMAN: Free to organize.
5 That's not what I said.

6 MR. SCIOLI: You know, at the --
7 earlier today Jim gave me, oh, a handsome
8 list of 25 names, 15 of whom are new names
9 for me. And Jim and I haven't compared
10 notes; maybe they're all familiar to him.
11 But we'd certainly look at this as a leaping
12 -- jumping off point, maybe leaping off
13 point, today and tomorrow morning.

14 The post-doc notion is intriguing
15 if in fact it's enticing to the student and
16 if we can make a substantial investment so
17 that it -- you know, someone who's thinking
18 about beginning a career is now willing to
19 do an extra year or 2 years. I'm fascinated
20 about the medical students who are willing
21 to do this, because they already have to
22 start thinking about careers.

1 MR. BRADBURN: — had already
2 put 5 years in his residency and put 2 more
3 years in as a research -- but they realize
4 that their 5 years of training give them
5 practically a trained incapacity to do
6 research.

7 MS. EAVEY: Well, it means --
8 perhaps we could think about facilitating an
9 infrastructure for the post-docs as opposed
10 to just funding post-docs. And I think
11 there's something up in Math that would be
12 an example, but I'd have to research it.
13 You know, one thing that NSF can do in
14 addition to supporting post-docs is we can
15 facilitate areas of research. Two areas
16 that the MMS program has highlighted over
17 the last few years have been surveys and
18 statistical methodology for surveys, in
19 conjunction with 13 or 14 federal
20 statistical agencies; and environmental
21 statistics in in conjunction with the EPA.
22 Are there research areas that

1 could be highlighted that would help to
2 bridge this gap?

3 MR. ACHEN: I think there are
4 packets of this kind. And I wonder whether
5 an integrated attack on specific problems by
6 people at several different age levels
7 isn't -- career levels isn't really the
8 point.

9 And I'm thinking a little bit here
10 about the MacArthur programs a few years ago
11 in international relations. We had a --
12 they spent some money on buying out the time
13 of people who'd been tenured a few years to
14 bring them into international relations,
15 people who'd had a side interest in IR but,
16 you know, had had to do something else up to
17 tenure time.

18 They also had post-docs and they
19 also had graduate students. So the -- there
20 was a bay in the -- we called these little
21 areas of ISR "bays." There was a little bay
22 with one of these ISR programs in it funded

1 by MacArthur. And one of those graduate
2 students teaches at Harvard, one of them
3 teaches at Princeton, one of them,
4 post-docs, teaches at Chicago, and so. It
5 threw a bunch of people together in a room a
6 lot smaller than this, gave them offices
7 around the sides, and a more or less common
8 set of interests. And they just talked and
9 argued all the time.

10 I wonder whether not breaking this
11 up into, you know, one program for this and
12 another program for that and another program
13 for something else, but rather thinking
14 about it as an integrated setup where a lot
15 of people are thrown into physical
16 proximity, some of whom might be 10 years
17 out and some of whom might be just
18 graduating -- whether something like that
19 might not be the right model for this,
20 rather than, say, three separate programs --
21 one for fellowships, one for post-docs, and
22 one for mid-career retraining.

1 MR. SCIOLI: Can we accept, then,
2 in this group -- and although a majority of
3 you are political scientists -- that in fact
4 our brethren in other social science
5 disciplines really don't have any upper hand
6 on us, at least the political science. You
7 know, I keep pushing the question, can we
8 learn anything from economics -- are they
9 doing this better? Because heaven knows,
10 they tell us they are.

11 MR. SIMON: Well, they are doing
12 -- I mean, in order to get this done well,
13 one needs -- this is about putting formal
14 theory and empirical theory together.

15 MR. SCIOLI: Yes.

16 MR. SIMON: And in order to put it
17 together, you have to do both well. And I
18 would say I think economists do the formal
19 theory better, and maybe the empirical
20 modeling better in some cases. And so --

21 MS. ZINNES: Their data may be
22 better.

1 MR. SIMON: Their data's -- it's
2 easier. I think they do it better because
3 it's easier. So, you know, maybe a part of
4 it is improving those pieces before we put
5 them together. But I don't think economists
6 put the two together that much better -- a
7 little bit, but not much.

8 There's a little bit of a -- there
9 had been a little bit of a informal
10 tradition in the Michigan econ department
11 that a good thesis would have both an
12 empirical and a modeling -- a good
13 theoretical thesis would have an empirical
14 piece. It's probably still there.

15 There should be ways of giving
16 incentives. I mean, any department that --
17 you know, NSF could give out fellowships and
18 to those departments that do a good job in
19 putting out theses that had both components,
20 for example.

21 MR. YOUNG: I don't know of
22 anything in economics that's along these

1 lines, really, that integrates the two
2 sides.

3 Santa Fe runs summer schools,
4 which are quite successful. They're a
5 little eclectic. But it's reasonably
6 competitive. I think students like to have
7 that on their vitae. It's not that time-
8 consuming. I mean, you devote a -- you go
9 out there for 2 months. A crash -- you
10 know, a sort of an intensive course for 8
11 weeks can accomplish quite a lot, actually.

12 MS. ZINNES: Intensive course on
13 what, for example?

14 MR. YOUNG: Well, that would be
15 tailored to the question here that we're
16 talking about, not what Santa Fe -- Santa Fe
17 does their thing. And actually their thing
18 turns out to be many different things. I
19 was thinking more of taking that general
20 model of a selective summer school that just
21 becomes known to be a place where good
22 students go because, you know -- so you have

1 to make it selective.

2 MR. KEECH: Russell Sage runs a
3 behavioral economics workshop summer camp
4 every other year, I think.

5 MS. MORTON: I think economics
6 does better, for the following reasons: I
7 think that in the graduate courses, the
8 substantive courses are always taught with
9 formal models in them. And when you take
10 econometrics, you begin with saying, okay,
11 we're going to use this statistical
12 technique to test formal models. I mean,
13 this is given and it's always there. And
14 you don't -- you're not taking courses where
15 it's not a given. And I think that this is
16 a very low-level.

17 It's so implicit in the way
18 economics does things that we don't even --
19 you don't even see it. But it's not there
20 in political science. And I don't know how
21 to get it there, but I think that that's the
22 big difference between economics and

1 political science. And you may be unhappy
2 with economic theory, Peyton, but the fact
3 is, is that there is theory taught in every
4 course that you take in economics. That's
5 formal.

6 MR. YOUNG: Well, but economics
7 teaches us the concept of opportunity costs,
8 and so what I'm worried about is that you
9 guys are going to say that, okay, well,
10 let's beef up the statistical training or
11 something for our students. Now,
12 something's -- I don't seriously believe
13 that you can extend the program by a year.
14 The opportunity cost, then, is that
15 something's going to give; they're not going
16 to take something that they now do take. So
17 then the question is, are you really gaining
18 from this?

19 My worry is that you're focusing
20 too much on the high-technique,
21 sophisticated training, it's true, that most
22 economic graduate students do get these

1 days, but the cost is huge. And I'll tell
2 you what one of the costs is. Most of these
3 students, including my own students, don't
4 know a thing about the real economy -- not
5 one thing. They don't know what the
6 unemployment rate is, they couldn't get
7 themselves out of any policy box you put
8 them into. I sense that political science
9 students at least know how Congress operates
10 more or less, and so forth and so on. That
11 is, you do pay attention to training them in
12 basic institutions.

13 And so I really worry that you
14 shouldn't go down this road at all. I mean,
15 I think economists have gone completely off
16 the deep end in not training students who
17 know broadly about the subject matter at
18 hand, in the real-world sense. They just
19 literally don't know. The worst offenders
20 are actually the econometricians. This is
21 really unbelievable.

22 MS. MORTON: It's been a long time

1 since I was in graduate school. I certainly
2 knew the unemployment rate then.

3 MR. BRADBURN: Let me mention
4 another model which goes in the opposite
5 direction, but it would speak more to the
6 issue that Peyton just raised, is the model
7 that mathematicians have called vertical
8 integration of graduate education. What
9 they do is they reach down to get
10 undergraduates involved, and so that think
11 of the graduate training program as being,
12 as a reaching down into the undergraduate --
13 so you're getting them started earlier, in
14 that sort of sense.

15 You've looked into it. Do you know
16 exactly how it works? Or you probably know
17 about it, Cheryl.

18 MS. EAVEY: Actually, I know about
19 it but I don't ----. I can tell you
20 tomorrow.

21 MR. BRADBURN: But I know that
22 this is a program that the NSF ---- program

1 does fund in institutions. It's getting the
2 undergraduate students to work essentially
3 with graduate students and faculty much
4 earlier.

5 MR. STRAF: And post-docs --

6 MR. BRADBURN: And post-docs, yes.

7 MR. ALDRICH: There's one at Duke.
8 And it does -- you're right -- the problem
9 is, they actually have seminars that are --
10 the teaching part of it -- have essentially
11 graduate seminars that are half
12 undergraduates and half graduate students.
13 Our problem is the undergraduates are
14 smarter than the graduate students, and
15 they're specialized.

16 MR. BRADBURN: That's I think true
17 at most elite universities.

18 MR. ALDRICH: I was going to go in
19 a slightly different direction, which is to
20 say that it seems to me that the most
21 important thing that NSF and, hopefully,
22 people like us can do is send a signal

1 symbolically of what's valued. I mean, I
2 think that, for example, a successful
3 political methodology section has instructed
4 a lot of people who go through graduate
5 school now as to what good methodology
6 consists of -- for good or ill. And some
7 good -- I mean, a lot of good and some ill.
8 Even if they never attend these things or
9 whatever, it just symbolizes.

10 And so we might want to think
11 about mechanisms that would send this kind
12 of signal as to what top-end, high-quality
13 research is, and that might mean doing
14 something like -- akin to that approach, but
15 just a program designed around, you know,
16 putting together theory and method.

17 MR. SCIOLI: Well, I glad that you
18 said that because I was reluctant to raise
19 it, but I was wondering is there a sense
20 that we shouldn't send a signal? I'm glad
21 you articulated it specifically, that this
22 group, who have absorbed the argument and

1 who -- you know, maybe there's too much
2 consensus around here, but the sense is that
3 there's something that should be done. And
4 whether it's analogous to the methods
5 workshop -- maybe the methods workshop is a
6 thing of the past now. Maybe it's too big
7 and, you know, we ought to think about
8 canceling the methods workshop. No more of
9 those plush meetings in hot climates and
10 dorm rooms of the Atlanta University Hilton
11 or whatever.

12 MS. MORTON: But I hear the year
13 after it's in Seattle.

14 MR. SCIOLI: Pardon me?

15 MS. MORTON: I hear it's going to
16 Seattle after that.

17 MR. SCIOLI: We're trying to talk
18 Bob Erikson (?) into having it in New York.

19 In any event, Jim and I talked at
20 the break, and we have a surprise. We're
21 going to break. We're going to go away,
22 because we're all -- now. Early dinner

1 instead, at 5:30 or 6:00. Because we have a
2 lot -- I think the sense is that you all can
3 build on this at dinner and that it's been a
4 long day, and some of you have come a long
5 way. And that the best way for us to get
6 business done efficiently in the morning is
7 to have some of this turn into after-meeting
8 discussions, et cetera.

9 Now, does anyone object to that,
10 to break here?

11 MR. BRADY: Can I recommend that
12 the list that was handed out -- it might be
13 useful for people to look at that tonight
14 and just check off things they see as
15 useful, things they see as useless.

16 MR. SCIOLI: Tell us what the list
17 is so that --

18 MR. BRADY: Well, it's a list that
19 I culled from the discussions and from all
20 the papers. And I tried to put next to the
21 proposals who I think made it a proposal. I
22 hope I haven't missed anybody. I'm sure I

1 screwed up -- my apologies.

2 But it is two kinds things:

3 Substantive —, which are sort of how do
4 we change the substance of what we're doing;
5 and then sort of process/procedural kinds of
6 things that were proposed. And see what we
7 think.

8 MR. ALT: It's a terrific service
9 you've --

10 SPEAKER: Thank you.

11 MR. BRADY: — start to tomorrow
12 as to what things will be eliminated, what
13 things do we want to focus on, and go from
14 there.

15 (Whereupon, at 5:17 p.m., the
16 PROCEEDINGS were adjourned.)

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